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THE

AMERICAN JOURNAL OF SCIENCE

[THIRD SERIES.]

ART. XXI.—*Niagara and the Great Lakes;* by FRANK BURSLEY TAYLOR.

Introduction.

IN the recent papers of Professor J. W. Spencer* and Mr. Warren Upham† the post-glacial history of the Great Lakes has been ably told according to two very different ideas of the cause of Pleistocene change. Prof. Spencer on the one hand levels all the higher abandoned beaches with the sea, and does not distinctly recognize a single ice-dammed lake. Mr. Upham, on the other hand, ascribes nearly all submergence to ice-dammed lakes, and admits none as marine except that which is proved by fossils. As often happens in such cases, the probability is that the truth lies between these wide extremes. Ice dams have played an important part, but not to the exclusion of marine submergence even at high levels. On the other hand, marine invasion is not available as an explanation for some of the most important areas of submergence.

The St. Lawrence river and the Great Lakes with their connecting channels are really all one stream. The lakes are great reservoirs which feed the rivers below them, and because they derive nearly all their supply from the lakes the rivers

*"The Duration of Niagara Falls," by J. W. Spencer, this Journal, Dec., 1894; "A Review of the History of the Great Lakes," Am. Geol., vol. xiv, Nov., 1894.

†"Late Glacial or Champlain Subsidence and Reëlevation of the St. Lawrence River Basin," by Warren Upham, this Journal, Jan., 1895; Twenty-second Ann. Rep't Geol. and Nat. Hist. Survey of Minn., Part III, pp. 54–66; "Departure of the Ice-Sheet from the Laurentian Lakes," Bull. G. S. A., vol. vi, 1894.

themselves have almost no independent existence. If anything happens to the lakes to turn their discharge in some other direction the rivers go nearly or entirely dry. Niagara is one of these rivers, and its history is inseparable from that of the lakes above it. Prof. Spencer has described the salient features of the Niagara gorge, and has also given many important facts bearing on the lake history. But certain facts which he does not take into account indicate a somewhat different lake history, and in consequence a different Niagara history also. The lake history is recorded in the larger characters, and it seems best therefore to study it first. Reference will be made in the following pages to six papers in which the writer's observations on the abandoned shore lines of the upper lakes are recorded.* Another paper discussing the latest chapter in the history of the Great Lakes also belongs to this series. It is entitled, "The Second Lake Algonquin."† It precedes this paper in order, and relates to the lake stages following next after those discussed here. These two papers together cover, in a preliminary way, the whole period from the final disappearance of the great Laurentide glacier down to the present time. But they do not include, except by incidental reference, the period of the glacial recession with its lakes. The map which accompanies this paper is designed to show within its limits the probable distribution of land at the maximum of marine submergence, and also the extent of that part of the first Lake Algonquin of which shore lines still remain.

The Three Principal Beaches.

After the glacial recession the three principal critical stages in the recent history of the upper Great Lakes are marked by three great abandoned beaches. Two of these are lake beaches and one is marine. The lake beaches mark the highest stages of two independent epochs of Lake Algonquin, which had an outlet on each occasion eastward across the Nipissing pass at North Bay, Ontario. One epoch of this lake existed before the marine invasion and the other after. The latest one I have called the second Lake Algonquin, and its highest shore

* 1. "Highest Old Shore Line on Mackinac Island," this Jour., III, vol. xliii, March, 1892; 2. "The Ancient Strait at Nipissing," Bull. G. S. A., vol. v, 1893; 3. "A Reconnaissance of the Abandoned Shore Lines of Green Bay," Am. Geol., vol. xiii, May, 1894; 4. "A Reconnaissance of the Abandoned Shore Lines of the South Coast of Lake Superior," Am. Geol., vol. xiii, June, 1894; 5. "The Limit of Postglacial Submergence in the Highlands East of Georgian Bay," Am. Geol., vol. xiv, Nov., 1894; 6. "The Munuscong Islands," Am. Geol., vol. xv, Jan, 1895. These papers will be referred to hereafter by number.

† Am. Geol, vol. xv, Feb. and March, 1895.

— SKETCH MAP —
SHOWING APPROXIMATE LIMITS OF
— POSTGLACIAL MARINE —
— SUBMERGENCE —
IN THE VICINITY OF THE GREAT LAKES
ALSO PART OF THE
— FIRST LAKE ALGONQUIN —
BY F.B. TAYLOR.
— REFERENCE. —
IROQUOIS, CHIPPEWA AND HERMAN BEACHES
CONJECTURAL EXTENSIONS.
ALGONQUIN BEACH
" " SUBMERGED
A.A. APPROXIMATE NODE LINE OF CHIPPEWA
AND ALGONQUIN PLANES.
B.B. APPROXIMATE NODE LINE OF ALGONQUIN
AND HURON-MICHIGAN PLANES
HUDSON BAY
GULF OF WINNIPEG
LAKE WINNIPEG
WINNIPEG
MISSOURI R.
BAYFIELD I.
NORTH BAY
GEORGIAN BAY
LAKE ALGONQUIN
FIRST LAKE ALGONQUIN
LAKE ERIE
ST. LAWRENCE R.
LAURENTIAN
ADIRONDACK
HUDSON-CHAMPLAIN STRAIT
BAY OF IROQUOIS
BUFFALO
DETROIT
PORT HURON
CHICAGO
MILWAUKEE
DULUTH
MISSISSIPPI R.
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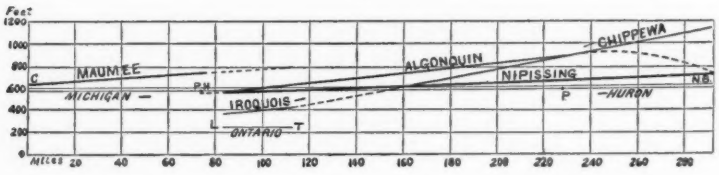
line is the Nipissing beach. Next before its time there was a marine invasion which made what I shall call the Chippewa beach. This beach marks the highest level of Warren Gulf, which was an arm of the sea. Before the time of the Chippewa beach there was the first Lake Algonquin, which was very much like the second lake of that name. Its highest level is marked by the Algonquin beach. This beach was made under substantially the same circumstances as the Nipissing. Before the beginning of the first Lake Algonquin there was a considerable period during which the upper lakes outflowed just as they do to-day, by way of the St. Clair and Niagara rivers. I call this the first Niagara stage of the lakes to distinguish it from the present, which is called the second Niagara stage. The earlier beaches of the first Niagara lakes were made at comparatively low levels in the north and were entirely obliterated by the first Lake Algonquin.

The three great beaches named in their order are the Algonquin, Chippewa and Nipissing. Changes of land attitude occurred in the long interval between the two Algonquin lake epochs, and caused the difference between the present places of the Algonquin and Nipissing beaches. The following ideal section shows, in a general way, the relation of the principal beaches.

1.



C=Chicago, Fort Wayne, etc.; P. H.=Port Huron; P.=Petoskey; N. B.=North Bay; L.=Lewiston, and T.=Toronto. The Maumee beach may be taken to represent all the glacial lake beaches above the Algonquin plane. The latter plane rises from 20 feet below lake level at Port Huron and after passing under the Chippewa beach descends to North Bay. The later Nipissing beach connects the same points, but without having any marked unequal deformation. The Iroquois and Chippewa beaches are shown as one continuous plane, which they probably are, although the connection from Belleville to North Bay has not yet been traced.

These statements present a brief outline of the lake stages and the relations of the beaches, but it remains to discuss the facts which tend to establish them as valid conclusions.

The Chippewa Beach.

When the water stood at its highest level, whether it was a glacial lake, or an arm of the sea, it made a continuous strand in one form or another along the entire extent of its land margin. When that beach is determined at frequent intervals with care it is fair to assume that it is continuous. If the beach has characteristic features which suggest identity this inference is greatly strengthened, and is not to be given up without positive proof of discontinuity. As a matter of fact the highest beach in the north is generally strongly developed and shows by its structure that wave action, at and near its level, was powerful and of long duration. Its ridges could have been made only by the heavy surf of a large body of water. At many places the upper limit of submergence is marked by a compact series of strongly developed shingle or gravel ridges with lagoon hollows between, sometimes covering altogether a horizontal width of a quarter to a half mile and usually measuring 35 feet from the crest of the upper ridge down to that of the lowest of the series. This arrangement is a very persistent feature of this beach and appears at widely separated places. It is typical at Mackinac Island and Sault Ste. Marie and is only slightly less conspicuous at North Bay, Marquette, L'Anse, Burnt Bluff and other places. Its similarity in all these places points strongly to its unity as one continuous beach conditioned at all places alike by a very even and gradual recession of the waters for the first 35 feet. In other places where the ridge series is not so conspicuous or is absent there is sometimes equally impressive evidence of long duration of submergence near one level in great gravel and sand deltas like those near Marquette, Cartier, and Sault Ste. Marie, or in deep deltoid deposits of laminated silt like those at Bracebridge and Burk's Falls, Ontario. The character of the beach and its situation on the various coasts of the north seem to leave no reasonable doubt that it is all one beach and marks the margin of one water plane.

In the first of the six papers mentioned above, I called the highest old shore line on Mackinac Island the Algonquin beach, supposing it to be the same as Spencer's beach of that name on the east Huron and Georgian Bay shores. But I afterwards found reason to doubt this correlation, and so, pending a fuller knowledge of the facts which might enable me to make a better judgment, I henceforth adopted the common term "highest beach" and used that without attempting to establish identity with other beaches already named. But observations have been considerably extended in the last two

or three years and enough facts are now at hand to form the basis of a much better correlation.

The highest beach of the north is not all of one age. A straight line drawn through Petoskey and Parry Sound, as shown on the map by AA, marks the approximate node line or place of division of the highest beach in two parts of different ages. South of this line the highest beach below the glacial lake beaches is all of one age, and is in fact Spencer's Algonquin beach. North of the line the highest beach is all of another age, and I propose to call it the Chippewa beach.* The reasons for the division on this line will be discussed farther on.

The Chippewa beach borders all the shores of the Great Lakes north of the Petoskey node line. In the following table its altitude is given at all places so far determined. The heights are in feet above sea level.

SUPERIOR BASIN.

Duluth, Minn.† (Lawson)	1134
Mt. Josephine, "	1207
Holyoke‡ (Upham)	1122
Maple Ridge, Wis.	1135
Kimball	1170
L'Anse, Mich.	1190
Marquette	1190
Sault Ste. Marie, Ont. (Lawson)	1014

MICHIGAN-HURON-GEORGIAN BAY BASIN.

Cook's Mill, Mich.	750
South Bay Hill	715
Burnt Bluff	705
Middle Munuscong Island	860
Mackinac	785
Petoskey	680
Cartier, Ont.	1200
Nelson's, near North Bay	1140
South River	1220
Sundridge	1205
Huntsville	1000

* I have long believed this beach to be one with the Iroquois beach of the Ontario basin. Reasons for this opinion will be set forth later. But inasmuch as this supposition has not yet been established by observations, it would seem premature to call it the Iroquois beach at the present time.

† "Sketch of the Coastal Topography of the North Side of Lake Superior with special reference to the Abandoned Strands of Lake Warren," by Andrew C. Lawson, Twentieth Annual Report, Geol. and Nat. Hist. Survey of Minnesota.

‡ Thirty miles southwest of Duluth. See Twenty-second Report Geol. and Nat. Hist. Survey of Minn., Part III, p. 59.

In the Superior Basin.—The Chippewa beach rises in 140 miles from 1134 feet above sea level at Duluth to 1207 feet at Mt. Josephine, near Grand Portage on the north shore, and in 172 miles to 1190 feet at L'Anse. Then from 1190 feet at Marquette in about 150 miles it drops to 1014 feet at Sault Ste. Marie. Cook's Mill is close to the north shore of Lake Michigan and is about 40 miles south from Old Munising. Along the comparatively low south shore east of Marquette we can only conjecture the probable height of the beach. Ground near Old Munising was washed over up to at least 250 feet. Scaffold Hill south of Munising station, rises a little higher, but has not been examined. If the beach rises northward from Cook's Mill at the same rate that it does from Mackinac to Sault Ste. Marie it would be about 340 feet above Lake Superior (940 feet above the sea) at Old Munising. Then from the Chippewa beach five miles west of Marquette to this point the descent would be about five feet per mile eastward.

On the north Superior shore, besides at Duluth and Sault Ste. Marie, the Chippewa beach has been reported probably at only one place, on Mt. Josephine at 1207 feet. The following are the principal high beaches observed by Lawson on the north Superior shore given in their order from west to east, Lake Superior being 602 feet above sea level. Those marked (*h*) are probably the Chippewa or highest post-glacial beach.

	Feet above Lake Superior.
Duluth, West, (*h*)	534
Duluth, 10th Ave., East, (*h*)	534.8
Hardy's School House	509.5
Two Harbors, D. & I. R. R.	475
Grand Portage*	458
Mt. Josephine, (*h*)	607.3
Kaministiquia (great delta)	455.1
Jackfish Bay "	418.3
Sault Ste. Marie, (*h*)	413.9

Jackfish Bay is the farthest north. But the observations of Lawson appear to show the Nipissing beach on the north shore at about 100 feet above the lake. It follows that the Champlain uplift raised this beach and necessarily all others lying above it relatively 125 feet from Duluth where the Nipissing beach is now submerged about 25 feet. The Nipissing beach rises northeastward a little over six inches per mile. The Chippewa beach rises from Duluth to Mt. Josephine about seven inches per mile. If the Chippewa beach continues in the same plane to Jackfish Bay its height would be about 1260 feet above sea level, or about 340 feet above Lawson's highest recorded beach at the latter place.

* Prof. Lawson does not give this terrace in his table.

On the extreme north side there are two passes across the Height of Land to the basin of Hudson Bay. That at Kenogami Lake is said by Lawson to be 1102 feet above sea level, and the other, at Lake Missinaibi, 1040 feet. Unless it descends from Mt. Josephine 100 feet in 140 miles northeast, the Chippewa beach must be higher than the Kenogami pass, and a strait must have existed there to Hudson Bay; and unless it descends 60 feet more in 150 miles east to the Missinaibi pass, that too must have been a strait. From Mt. Josephine to Sault Ste. Marie around the north side we have no probable record of the Chippewa beach. The beach at the former locality is 183 feet higher than at the latter, and at both places it is inclined upward to the north. If the extensions from these two places unite in the north as one beach, it is obvious that the part extending northward from Sault Ste. Marie must rise more rapidly than the other. This supposition is supported by the facts in each locality. The northward rise from the Middle Munuscong Island to Sault Ste. Marie is over four feet per mile, while that northward from Duluth to Mt. Josephine is a little over one foot per mile. The pass at Lake Missinaibi is only 26 feet higher than the Chippewa beach at Sault Ste. Marie, and the distance is about 120 miles due north.

These considerations are not proof. But they are strong indications, and they are the best we can do in our present state of ignorance as to the place of the Chippewa beach on the north Superior shore. The strait of that time connecting Lake Superior with Lakes Michigan and Huron was about 110 miles wide.

In the Michigan-Huron-Georgian Bay Basin.—At the time of the Chippewa beach these three basins were all united together and with the waters of the Superior basin as one great gulf with only a few islands to break its entire stretch from Duluth and the west side of Green Bay to the highlands east of Georgian Bay. Southward the Chippewa beach intersects the Algonquin beach along the Petoskey node line, and the two appear to be physically continuous. The most critical extension of the Chippewa plane in these lower basins is toward the northeast from Lake Huron and Georgian Bay. In that direction it passes high over two divides, making straits to the Ottawa valley, with probably another beyond to the basin of Hudson Bay.

From 1014 feet at Sault Ste. Marie the beach rises to about 1200 feet at Cartier (130 miles east) and 1140 feet at Nelson's, five miles northeast of North Bay. From 1000 feet at Huntsville the beach rises to 1220 feet at South River (35 miles north), and probably a little higher at Trout Creek, 11 miles

farther north and about 28 miles south of North Bay. This shows a descent of something over 80 feet from Trout Creek to the Chippewa beach at Nelson's—a rate of over two and a half feet per mile. This might suggest that the plane passes downward toward the north indefinitely so as to pass below the Abitibbi pass (957 feet). But at Cartier, which is about 25 miles farther north than North Bay and about 100 miles to the west, the height of the beach is about 1200 feet. Perhaps this leaves some uncertainty as to the existence of the Abitibbi strait to Hudson Bay, but it makes that over Lake Tamagaming to the upper Ottawa valley almost certain. The Nippissing strait was about 25 miles wide and nearly 500 feet deep, while the Tamagaming strait was probably between 300 and 400 feet deep and about 50 miles wide. The continuity and oneness of the whole Chippewa beach, stretching as the highest shore line from Duluth to North Bay and on all the shores north of the Petoskey node line, seems to be a certainty. It is believed that this great expanse of water was an arm of the ocean, and following previous usage as closely as possible, I call it Warren Gulf.*

From the Petoskey node line the Chippewa beach undoubtedly extends southward, but in all three basins it passes below the Algonquin plane, and unless it bends upward must strike far below the level of both the St. Clair and Chicago outlets. The Chippewa plane produced south from Burnt Bluff at its descent of two and one-third feet per mile from Cook's Mill passes under the lake on the Michigan shore close to Cave Point, about ten miles north of Sturgeon Bay and about at Peshtigo Point on the west side of Green Bay. The great littoral accumulations at a low level near the former place, and the great Menominee-Peshtigo delta at the latter, seem to give some support to this supposition. Continued to Chicago at the same rate the plane would strike over 350 feet below lake level.

The Chippewa beach marks the highest stage and greatest extent of Warren Gulf. For it is the highest post-glacial beach in the north. In one of his recent papers† Prof. Spencer describes several beaches at much higher levels. He supposes the extension of the deserted beaches of the Erie basin to the Adirondacks, and to other distant northern regions. It evidently seemed necessary to him to postulate these extensions because he takes all the beaches to be of marine origin.

* Spencer at first called the expanded lakes "Lake Warren," although he recognized that it might be an arm of the sea. Later he adopted the less specific term "Warren Water." But he does not recognize its limits exactly as they are here defined.

† "High Level Shores in the Region of the Great Lakes, and their Deformation," this Journal, vol. xli, March 1891.

On that hypothesis such extensions seem inevitable. But this assumption is not borne out by observation. There are no beaches in the north corresponding to those of the Erie basin.* In exploring those regions I endeavored always to locate the *highest* beach. But I found no evidence of submergence above the Chippewa beach anywhere in the north. I did not find a single feature that could be attributed with certainty to wave action, and that is the crucial test for widespread submergence.

Prof. Spencer gives some prominence also to other supposed beaches at much higher levels.† The facts upon which their identity rests, however, are very scant and scattered. The basis for the broad conclusions which Prof. Spencer draws from them, following Dawson nearly to the utmost in his idea of widespread submergence at very high levels, seems to me to be altogether inadequate. For it is merely a promiscuous collection of random observations in which occasional features more or less resembling beaches, terraces or other littoral forms were observed by different persons at widely separated places. The characters which these observations record are not sufficiently clear to identify the shore line of a great expanse of water. Forms which show only general resemblance to beaches, terraces, etc., are not enough. Glacial and glacio-fluvial action make all these in abundance. The Erie and Michigan beaches and others above the Iroquois plane will probably all be finally correlated with moraines.

Warren Gulf, therefore, did not extend to the Erie basin, although it filled all the others above. Prof. Spencer has defined "Lake Warren," or "Warren Water," as the predecessor of "Lake Algonquin." On this account it has seemed best to apply the name to the water which made the Chippewa beach rather than to the glacial waters which made the Maumee beach of the Erie basin.

The Gulf of Winnipeg.

If there was a Warren Gulf there is the strongest reason to believe there was, contemporaneously with it, a Gulf of Winnipeg, covering the area commonly spoken of as the basin of

* Mr. Upham, in the papers cited above, correlates the Nelson beach near North Bay with the Belmore beach of the Erie series in Ohio. Between Nelson's and South River there are probably recent faults, so that from Nelson's the Chippewa beach rises to South River and from there it descends and is almost certainly continuous to Huntsville where it intersects the Algonquin beach. The latter is better known and is continuous to Grand Bend 40 miles northeast of Sarnia, where it is 18 feet above the lake and descending westward about one foot per mile. Spencer's data for calculating its place at 20 feet below lake level off Sarnia are good. It seems to me therefore that Mr. Upham's correlation is impossible.

† Op. cit., pp. 209–211.

the glacial Lake Agassiz. The subject is a little apart from the present paper and I can discuss it no farther than to point out the probable continuity of the Chippewa beach and that known as the Herman beach of glacial Lake Agassiz. From Duluth to L'Anse the Chippewa beach rises about 56 feet in 172 miles, or about four inches per mile.* Mr. Upham shows the highest beach of "Glacial Lake Agassiz" reaching up to Lake Naquauquon at the west end of Hunter's Island on the international boundary.† This lake is 1186 feet above sea level and is about 140 miles west and 25 miles north from Mt. Josephine and 120 miles straight north of Duluth. On the best present data the Chippewa plane produced from these places strikes about 20 feet below its level. This lake is about 75 miles from the nearest point on the Superior shore.

From Mt. Josephine to the east side of Beltrami Island is about 250 miles a little north of west. The highest beach at Mt. Josephine is 1207 feet above sea level and that on Beltrami Island 1215 feet, and Lake Naquauquon lies almost directly between. The Chippewa plane carried westward strikes 50 to 100 feet below the Herman beach in the valley of the Red River of the North.

The Herman beach rises from 1045 feet at Lake Traverse to nearly 1170 feet at Maple Lake and 1215 feet on Beltrami Island, and to something more than 1186 feet at the west end of Hunter's Island; on the west side to 1226 feet at the international boundary in Dakota and to 1460 feet near the northwest corner of Manitoba. The altitude of Rainy Lake above sea level is 1117 feet and of the Lake of the Woods 1060 feet. About 200 miles northeast of the latter is the Height of Land and the divide between Lac Seul and Lake St. Joseph. The altitude of the former is 1232 feet and of the latter 1172 feet. Lake Nipigon is 915 feet above the sea. From this last lake it is only about 50 miles east to Kenogami Lake and the pass at 1102 feet. The Herman beach in Minnesota, Dakota and Manitoba rises steadily northward. The divide between Lac Seul and Lake St. Joseph is about 150 miles north and the same distance east from Beltrami Island where the beach is

* Its plane produced passes about 60 feet over the pass at Upper St. Croix Lake, which is 468 feet above Lake Superior. This suggests that the waters that made the Chippewa beach were those of a lake with its outlet at this pass. No doubt there was an ice-dammed lake in the western part of the Superior basin during the glacial recession. But it does not follow that the highest beach found there to-day was made by that lake. It seems more probable that it was made by the waters of the later Warren Gulf. The apparent continuity of this beach with the rest of the highest beach eastward to North Bay makes marine origin more plausible. The marine invasion probably obliterated the remains of the glacial lake. The same thing probably occurred in the Ontario and Champlain basins and possibly also in the Winnipeg basin. In the last, however, there may have been only a great glacial river without any lake.

† Map with Mr. Upham's third paper referred to above.

1215 feet. Every probability suggested by a study of the abandoned shore lines of this region points to the inference that the beaches which pass north from Lake Superior and from Rainy Lake rise more or less toward the north. From Beltrami Island the beach need rise only 30 or 40 feet to clear the Seul-St. Joseph pass, and only 40 to 50 feet from Mt. Josephine.

It seems to me that the two planes in the Superior and Winnipeg basins come as near continuity and unity as could be expected where considerable unequal deformation is known to exist. Whatever the character of these waters they appear to be related to each other like different parts of the shore of one water body—as though their levels were both conditioned by the same cause, whether an outlet river or the plane of the sea.

The Hudson-Champlain Strait.

A glance eastward from Nipissing pass is still more suggestive of marine submergence at the time of the building of the Chippewa beach. Down the widening Ottawa valley to the St. Lawrence and thence to the sea and across by way of Lake Champlain and the Hudson valley the water must have stretched in one simple unbroken sheet unless there was not only an ice dam at the north, but a contemporaneous land elevation at the south.

For the maintenance of his glacial lake hypothesis Mr. Upham has found a pressing need to include the Hudson valley in his scheme. For if it were proved that the high post-glacial submergence in that valley was marine then the whole system of basins with which the Hudson valley connects toward the north, so far as relates to high post-glacial submergence, would have to be stricken from the category of ice-dammed lakes. Hence Mr. Upham had to suppose, besides an ice dam at the north, an extensive elevation of the sub merged coastal plain 50 to 100 miles southeast of New York and also off the east end of Long Island. Merrill has found the marks of post-glacial submergence rising northward from 75 to 80 feet above sea level at New York to 210 feet at Fishkill and 340 feet at Schenectady.* Davis's Catskill deltas lie in the same plane and Mather's sand plains extend up to the Champlain basin, where Hitchcock, Baldwin and the writer have traced high-level submergence up to the Canada line. This plane produced southward strikes sea level 40 to 50 miles south of New York, where the old submerged Hudson river channel is now over 200 feet under water. The passage southeast-

* "Post-glacial History of the Hudson River Valley," by F. J. H. Merrill, this Journal, III, vol. xli, June, 1891.

ward from Long Island Sound is about 100 feet deep, and the continental shelf all along slopes seaward. In order to make a land barrier for the water in the Hudson valley it is necessary to suppose a southward differential elevation of more than 200 feet 50 miles south of New York and of 100 feet off Montauk Point. But there is no evidence of such an amount of elevation, still less is there any evidence of differential rise to the south. The supposed post-Champlain elevation 100 feet higher than at present at New York and later re-submergence to the same amount, for which Merrill offers some evidence, can not serve Mr. Upham's purpose. For the glacial lake, if it ever existed, was long before the time of the Champlain submergence.*

The hypothesis of a southward land barrier of this ice-dammed lake seems to me to be in the face of the plain reading of the facts. The simplest interpretation is most likely to be true and in this case it is that the Hudson-Champlain trough was occupied at the highest stage of submergence by marine waters which connected with the Laurentian Gulf of that time. And this did not follow closely upon the retreat of the ice, but came at a time long after.

But if the sea filled the Hudson-Champlain trough, attaining a height of 700 feet† or more at the north end of Lake Champlain it is impossible to avoid the inference that this was the time of the formation of the Iroquois, Chippewa and Herman beaches. Considering the very recent occurrence of the Champlain uplift the absence of fossils in the much older, higher deposits can hardly be regarded as an important consideration in determining the character of those deposits.

The First Lake Algonquin.

The first Lake Algonquin was a post-glacial lake occupying the basins of the three upper lakes and having its outlet eastward over the Nipissing pass. Its area was a little larger than that of the second Lake Algonquin, for its shores are higher at all places save one at the head of the St. Clair river, where both planes come to one level.

The history of the second Lake Algonquin and the deformation of the Nipissing plane are fully discussed in the paper of that title referred to above. The facts at present in hand relating to that lake are much more numerous than those

* The very recent date of the uplift that brought this submergence to a close is shown in the paper on the second Lake Algonquin; and the lateness of the submergence itself is readily inferred from the sequence of lake stages set forth below.

† I found well formed beach ridges at 660 feet near West Chazy, N. Y., with less certain marks of submergence up to about 800 feet.

which relate to the first Lake Algonquin. So far as they go, however, the latter are almost as good. Only the southern parts of the Algonquin beach are now left, those at the north having been washed over and obliterated by the later Warren Gulf. All the facts now known relating to the first Lake Algonquin are found south of the Petoskey node line.

In the Michigan Basin.—The history of Lake Algonquin in this basin was very simple in both epochs. More than half of the deformation which is found in the Algonquin beach was produced by the great Champlain uplift which so recently deformed the much younger Nipissing beach.

As nearly as I have been able to determine, the node line of the Chippewa and Algonquin planes passes east and west through Petoskey, as shown on the map by the line AA. North of this line the highest beach throughout is the marine beach of Warren Gulf and rises rapidly northward. South of the line is the true Algonquin beach. In the field the two so far as yet observed appear to be physically continuous as one beach. But taking the plane as a whole there is a decided break near the line AA, and this I take to be the node. This break in the plane is clear on both shores of Lake Michigan, but is less noticeable in the highlands east of Georgian Bay. Northward from Petoskey the Chippewa beach rises to Mackinac about three feet per mile, while southward from the same place the Algonquin beach declines about seven or eight inches per mile. The altitude of the Algonquin beach at Traverse City was not determined accurately. But terraces have been reported at Frankfort on the Michigan shore farther south, corresponding very closely to the extended Algonquin plane.

On the west side of the lake the Chippewa beach rises northward from Burnt Bluff to Cook's Mill at the rate of about two and one-third feet per mile. The Algonquin beach was found to pass under present lake level at Two Rivers, Wisconsin. Southward from that place it appears as submerged terraces off shore at Manitowoc and Sheboygan. Northward from Two Rivers it was found at Kewaunee, Green Bay, Sturgeon Bay, and Birch Creek north of Menominee. Its heights at all these places fall into an even plane which rises northward eight inches per mile. If this plane be produced northward to Cedar river on the west shore of Green Bay about 25 miles north of Menominee its altitude at that place would be about 60 feet above the lake. The Nipissing beach is calculated to pass under the lake at about the same place. Hence the Algonquin beach is there about 60 feet above the Nipissing. Cedar river is also near the node line AA, and is 120 miles straight west from Petoskey. At Petoskey the Nipissing

beach is 25 feet above the lake, and the Algonquin 100 feet. From these data it appears that in 120 miles from west to east the Nipissing beach rises 25 feet and the Algonquin beach 40 feet, or at rates of two inches and three and one-half inches per mile respectively. On the east side the Algonquin plane projected southward, passes under the lake in the vicinity of Ludington or Pentwater. On the west side the plane carried from Two Rivers to Chicago 150 miles south strikes more than 100 feet below the lake.

Mr. Upham supposes his glacial Lake Warren to have had its outlet at Chicago, and he refers to my third paper to show that the highest beach of the Green Bay region continues southward about at present lake level. He may have been misled by a misprint in the table of altitudes in that paper.* But I state distinctly in the concluding paragraph of the article that the beach passes under the lake southward from Two Rivers. If he accepts the present facts without offering others to controvert the conclusions which they plainly indicate, Mr. Upham will have to grant that neither the waters which made the Algonquin beach nor those which made the Chippewa beach had an outlet at Chicago. There are beaches converging to the Chicago outlet, as is well proved by the observations of Andrews, Bannister and Leverett. But they do not extend far north, and all are in planes which lie above the Algonquin, Chippewa and Nipissing planes. These latter planes all pass down under the lake long before they reach Chicago. The Algonquin reaches farthest south above lake level, and its plane produced strikes higher at the south end of the lake than either of the others.

It is interesting to note the relation of this part of the Algonquin plane to the two outlets which served the second lake Algonquin. From Petoskey to Port Huron is about 175 miles south and 125 miles east, and the plane projected from Lake Michigan passes 40 feet above the calculated place of Algonquin beach off Sarnia. From Petoskey, North Bay is about 260 miles east and 60 miles north, and the plane produced passes 210 feet above Georgian Bay, or about 50 feet above the Nipissing beach at the old outlet at North Bay. Subtracting 40 feet for post-Nipissing local elevation from the altitude of the pass at North Bay, the Algonquin plane produced strikes 90 feet above the outlet. Considering the fact that the Algonquin beach is the oldest of the three it is not surprising that its plane produced from so great a distance comes no nearer to these outlets.

*Third paper, second line in table on page 325, the altitude of the "Beach southward from Two Rivers, Wis.," was intended to read "—582" feet, not "582" feet.

The altitudes of the Algonquin beach at the different points of observation are given in the following table in feet above Lake Huron, which is 582 feet above sea level:

MICHIGAN BASIN.

Birch Creek	50
Sturgeon Bay	35
Green Bay	25
Kewaunee	15
Two Rivers	0
Manitowoc and Sheboygan	—0
Traverse City	70 or 80
Petoskey	100

HURON-GEORGIAN BAY BASIN.

Off Sarnia (Spencer) calculated	—20
Grand Bend "	18
Wilson's "	36
Eighteen Mile Creek (Spencer)	65
Southampton (Spencer)	132
Owen Sound "	167
Clarksburg "	191
Collingwood "	185
Colwell "	170
Elmsvale "	220
Wyebridge "	260
Midland (Taylor)	240
Orillia "	250
Barrie "	200
Lorneville "	235
Kirkfield (Spencer)	293
Gravenhurst (Taylor) approx.,	350
Bracebridge "	390
Huntsville "	418
Water level over the Nipissing pass	160

Prof. Spencer's heights were nearly all measured by leveling, those of the writer by aneroid barometer.

In the Huron-Georgian Bay Basin.—The eastern part of the Algonquin beach has been greatly deformed. But its relation to the St. Clair river points strongly to its origin as a lake beach. It has an eastward rise of about one foot per mile throughout. The northward rise is less at the south. At Grand Bend, which is about 40 miles northeast of Sarnia, the beach is 18 feet above the lake. The place of the beach (submerged 20 feet) off Sarnia was calculated on these data. It probably passes under the lake near Cape Ipperwash, which is about 25 miles northeast of Sarnia. On the opposite sides of Saginaw Bay there are two points, Sand point on the

east side and Gravelly point on the west, which may also be, as Spencer makes them conjecturally, nodal points. But this has not yet been established as a fact by observation.

From 20 feet below lake level at Sarnia to the Nipissing pass is a rise of 140 feet (40 feet, as before, deducted from altitude of Nipissing pass for recent local uplift). The northward distance being about 235 miles, the rate of rise is about seven inches per mile. Southampton is 125 miles north of Sarnia and 50 miles east. Kirkfield is 140 miles east and six miles north of Southampton. The Algonquin beach rises northward from Lorneville to Kirkfield 60 feet in 15 miles, and it descends from Kirkfield to Southampton 161 feet. Taking off 24 feet for the greater distance of Kirkfield north makes 137 feet descent westward in 140 miles to Southampton. This descent continued 50 miles farther west to the meridian of Sarnia passes 82 feet above the lake, whereas the Sarnia-Nipissing plane is 19 feet lower—63 feet above the lake at the same point. As compared with this plane the Algonquin beach is 69 feet too high at Southampton, 130 feet at Lorneville, 190 feet at Kirkfield, and 334 feet at Huntsville. The same comparison shows that the beach at Petoskey is only five or six feet higher, and that at Cedar river on the west shore of Green Bay is about 34 feet lower. The Algonquin plane produced from Cedar river and Petoskey to Huntsville, 275 miles east, would be 90 feet above the Sarnia-Nipissing plane. It seems certain that the Algonquin beach was originally formed very nearly in the latter plane, and the present departure from it is the measure of the amount of subsequent unequal deformation.

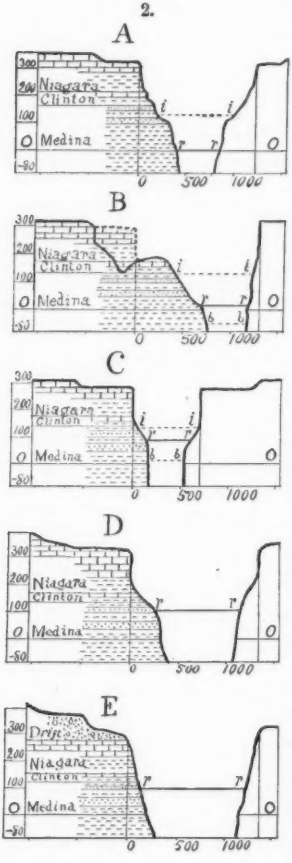
These facts show how great is the eastward element of deformation east of Lake Huron and Georgian Bay. The northward uplift appears also to be irregular, especially farther north, as shown by the Chippewa beach. All this deformation, less that due to the later Champlain uplift, appears to have occurred after the Chippewa beach and before the Nipissing.

This is the present sum of the facts which bear directly upon the first Lake Algonquin, so far as relates to the evidence in the upper lake basins, and if this were all the existence of the lake as one with a two-outlet stage like the second Lake Algonquin would remain in some doubt. But there are facts connected with the Niagara gorge and Warren Gulf which define the conditions which existed just before and just after the supposed first Lake Algonquin so clearly that theoretical considerations show that this lake must have existed in the interval. These will be presented briefly later.

The Niagara Gorge.

In his admirable paper on the "Duration of Niagara Falls," Prof. Spencer presents five cross sections of the Niagara gorge. A close comparison of them is so instructive that I have reproduced them in a single cut. Some of the details of his drawings are omitted. The significance of the difference between C and D and E was sufficiently explained in the paper on the second Lake Algonquin. The conclusion then reached was, that considering the uniformity of the geological structure it must be admitted that if D and E were made by the great cataract, C could not have been, but must have been made by a stream of much less volume. That stream was the Erigan river, draining only Lake Erie, which did not then receive the discharge of the upper lakes. Prof. Spencer's explanation, that C was made while the great cataract had a sheer fall of 420 feet was shown to be inconsistent with the obvious fact that the Erigan section of the gorge is much shallower than the wider section above.

2.



If this explanation of the origin of the Erigan gorge, which is represented by C, is valid, then the same argument applies to the lower section of the gorge which is represented by B and A. These sections must have been made by the great cataract and not by the Erigan Fall. For A and B are almost exactly the

NOTE TO FIG. 2.—These cross-sections are situated as follows beginning at the north or lower end of the gorge. A, half a mile above lower end; B, at Foster's Flat; C, at Whirlpool rapids just below the railroad bridges; D, at Johnson's Ridge about one mile above the bridges; E, at the Horseshoe Fall. The top of the sections is hard Niagara limestone. Then comes softer, shaley layers with the harder Medina below. OO, level of Lake Ontario; *rr*, river level; *ii*, level of Iroquois marine water during middle of Erigan epoch; *bb*, probable bottom of gorge (about 75 feet below *rr*). Both scales in feet. The strata are substantially the same on both sides of the gorge. These sections are copied from Prof. J. W. Spencer's cuts in his paper in this Journal for December 1894, with some omissions and also with some additions. Note the shallowness of C (bottom about at *bb*) and narrowness at top as compared with A, B, D or E.

same as D and E, the differences being no greater than those which may arise from the action of a river of constant volume. The argument for difference of volume is as strong by the comparison of A and B with C as in the other two sections. It seems certain that the upper lakes did not drain through the St. Clair river during the making of the Erigan section. The great cataract has therefore had two independent periods of activity separated by the Erigan period. This much seems clear from an examination of the gorge alone.

The level of the water in the Ontario basin has undergone several changes during the making of the gorge. At the beginning of the first Niagara period the water probably stood higher than now at Lewiston, but not long above the Iroquois level. Then it gradually fell away to a level 80 to 100 feet lower than to-day. All this was during the first Niagara period. The great cataract stopped either at Foster's Flat or at the upper side of the Whirlpool. There is still some doubt on this point, with probability, it now seems to me, in favor of the latter place.

From the Whirlpool up to a point a few rods above the cantilever bridge the gorge is narrow and shallow, and the latter character continues also below the Whirlpool to the lower end of Foster's Flat. It was presumably about at the middle of the Erigan period that the Iroquois beach was made at Lewiston, 135 feet above present lake level. The water at that time stood in the gorge at a level slightly higher than the water level at the foot of the present falls. This, with the smallness of the Erigan Fall, explains why this part of the gorge is shallow. After that the water fell away again to 80 or 100 feet below its present level during the early part of the present period of the cataract and then more recently was raised again to its present level by the Champlain uplift which elevated the outlet of Lake Ontario. These changes, with the harder quality and higher level of the Medina rocks in the bottom of the gorge, explain its salient peculiarities below the Whirlpool, and the changes in the volume of the river find their correlatives in the history of the upper lakes.

The Succession of the Lake Stages.

In the paper on the second Lake Algonquin it was shown that the whole interval from the present day back to the time of Warren Gulf is completely filled by the series of events there described. The gorge of Niagara from the falls to the cantilever bridge suggests no change or variation in the volume of the river. But at and below the bridge th gorge grows narrow and shallow. Considering the uniformity of the geolog-

ical structure, this feature is explicable only on the supposition that the great cataract was not then active, but was replaced by a much smaller one. That was the Erigan cataract.

In the lakes, the Nipissing beach, which is the latest abandoned shore line marking a critical stage, connects the St. Clair and Nipissing outlets, and was evidently made when both were flowing. But the Nipissing outlet is now 160 feet above Lake Huron, so that ever since that outlet was abandoned Niagara has had all the discharge of the upper lakes. The position of the Nipissing beach is such that the Chicago and Trent valley outlets are absolutely excluded. The Nipissing beach marks the activity of the St. Clair outlet and also that of the Nipissing outlet, but not of any other. In tracing back the order of changes we are therefore compelled to pass from the St. Clair to the Nipissing outlet, and so from the present lakes to the second Lake Algonquin. There is no escape from this step. This brings us to a time when Niagara did not receive the waters of the upper lakes,—the perfect correlative of the Erigan section of the Niagara gorge. But before the second Lake Algonquin there was Warren Gulf with its marine straits. The Erigan Fall replaced Niagara during that time also. The simplest possible supposition as to the cause of these changes, and it is the only cause which is indicated by the facts, is, that there has been going on throughout the whole period a very gradual differential elevation, greater always at the north than at the south. This elevation covers all the changes which have taken place since the Chippewa beach was at sea level, and tallies, we may say, with half the work done in the post-glacial Niagara gorge. That part of the gorge below the Whirlpool is similar to the part above the bridge, and suggests the existence of a pre-Erigan great cataract, substantially like the present one, which again suggests a pre-Warren lake stage, substantially like the present.

And for this we have only to suppose an immediately post-glacial condition of elevation at the north similar in a general way to the present land attitude and a progressive pre-Warren depression, to introduce in reverse order all the post-Warren changes. While that elevation existed the gorge was cut out from Lewiston to the whirlpool. The first marked change produced by the depression was the opening of the Nipissing outlet, at which the Niagara cataract began to weaken. Further progress of the depression closed the St. Clair outlet, and the erosion of the Erigan gorge began. The continuing depression next transformed the Nipissing outlet into a strait, and Warren Gulf was established. At the maximum of the depression the Chippewa marine beach was formed. The ero-

sion of the Erigan gorge thus covers the duration of the first Lake Algonquin, Warren Gulf, and the second Lake Algonquin.*

If this is a true statement of the order and character of the changes, we should expect to find the strongest confirmation of it in the most recent beaches with confirmations less and less distinctly marked as we go backward in time. And so it is. In the article on the second Lake Algonquin are detailed the facts which seem to show the existence and boundaries of that lake, and that it filled the epoch of lake history immediately preceding the present. In the present paper have been stated facts in support of the view that the Chippewa beach marks the climax of a great marine invasion which formed Warren Gulf, and facts which show with less distinctness, but with some degree of force, the existence of the first Lake Algonquin. To this it may be added that if we accept the enlarged gorge below the Whirlpool as evidence of a pre-Erigan discharge of the whole lake system by the Niagara river, it will necessarily follow that between that stage and the Warren Gulf epoch there must have existed a first Lake Algonquin marking the transition from the first Niagara lakes to the Warren Gulf epoch, just as the second Lake Algonquin marked the transition from the Warren Gulf epoch back to the second Niagara system.

Chronological Conspectus.

At its maximum the great Laurentide glacier covered the whole area of the Great Lakes. By a correlation of the abandoned shore lines, moraines and outlets, and the gorges, recently submerged shores and rivers of this region the following order of events is made out for the post-glacial history of the Great

* An idea of the probable slowness of the recession of the Erigan Fall and the time required to make its gorge may be gathered from the following words of Mr. G. K. Gilbert (Letter in Nature for May 17, 1894). After mentioning the several accurate surveys that have been made of the Falls, he says: "It has thus become known, first, that in the middle of the Horseshoe Fall, where the principal body of water descends, the brink retrogrades at the rate of four or five feet per annum; second, that the American Fall, carrying a much thinner sheet of water, retrogrades so slowly that its rate is concealed by errors of survey." Speaking of the post-glacial period when the upper lakes had their outlet in some other direction and Niagara was fed only by Lake Erie, he says: "During that epoch the volume of the river was so small that cañon-cutting was affected only by the feeble process now illustrated by the American Fall." This was the Erigan epoch. (See also Mr Gilbert's "History of Niagara River," Smithsonian Report, 1890). The brink of the American Fall is to-day almost exactly in line with the brink of the cañon wall above and below. While the Horseshoe Fall has receded from the center of the American Fall (about 3000 feet) the American Fall has receded at most not more than 100 feet and probably less than 50 feet, and it still strikes at its foot upon rock ledges and bowlders which it has not yet been able to remove. From the measured difference between the volume of the St. Clair and Niagara rivers it is found that the contribution of Lake Erie is about three-elevenths of the whole, and this, as Spencer states, is almost the same as the present volume of the American Fall. (See also L. Y. Schermerhorn, this Journal vol. xxxiii, April 1887.) The true Erigan section of the gorge is probably about three-fourths of a mile long.

Lakes. They are set down in seven principal stages with transitions or critical stages between.

I. Glacial, ice-dammed lakes. Outlets at Fort Wayne, Chicago and other places. Beaches correlated with moraines in Ohio. Glacial lakes fall by stages as outlets change on withdrawal of the glacier-dams. Land relatively high in the north but slowly subsiding.

First Transition: By withdrawal of glacier the Niagara river is opened and the upper lakes become united.

II. First Niagara lakes. First epoch of Niagara Falls begins at Lewiston. For a short time glacial Lake Iroquois receives the water from Niagara. Shore lines of lower levels of this glacial lake washed over and obliterated by later marine invasion. Gradual depression of land continues at north, finally opening Nipissing outlet.

Second Transition: First two-outlet climax. Marked by the ALGONQUIN BEACH. (Possible subdivision here for supposed Trent river outlet.) Gradual northward depression continues. First epoch of Niagara Falls closes at the Whirlpool. Epoch of Erigan Fall begins.

III. First Lake Algonquin. Outlet eastward over Nipissing pass.

Third Transition: Gradual northward depression continues. Nipissing outlet brought down to sea level. Lakes become marine.

IV. Warren Gulf (rising stage). Marine waters fill the three upper lakes, the Ontario, St. Lawrence and Winnipeg basins.

Fourth Transition: Marine climax. Marked by the CHIPPEWA BEACH. Northward depression ceases and gradual elevation begins. Iroquois and Herman marine beaches made at the same time as the Chippewa. This was probably the climax of the post-glacial warm epoch.

V. Warren Gulf (falling stage). Gradual northward elevation. Irregular uplifts in the north deforming Chippewa and Algonquin beaches.

Fifth Transition: Nipissing outlet raised to sea level. Upper lakes become fresh.

VI. Second Lake Algonquin. Outlet eastward over Nipissing pass. Probably a small amount of local uplift at outlet in early stage.

Sixth Transition: Second two-outlet climax. Marked by the NIPISSING BEACH. Epoch of Erigan Fall closes at a point between 40 and 80 rods above the cantilever bridge. Second (present) epoch of Niagara Falls begins.

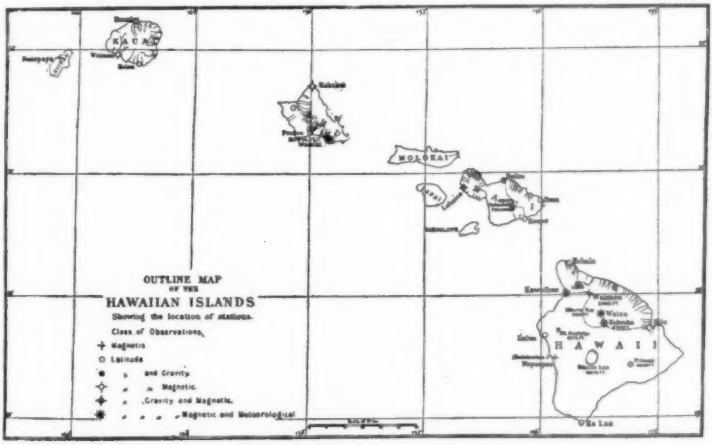
VII. Second Niagara lakes (present stage). Lake Superior becomes independent. Great Champlain uplift at the northeast. Formation of St. Clair delta begins and continues to the present time.

Fort Wayne, Ind., Feb'y 18, 1895.

Art. XXII.—*Disturbances in the direction of the Plumb-line in the Hawaiian Islands;* by E. D. Preston.

Professor Alexander, Surveyor General of Hawaii has made a comparison of the astronomical and geodetic latitudes on the three principal islands of the group. In addition to this, since the observed latitudes at Kawaihae, Hilo, and Waiau (Mauna Kea) are consistent with one another the latter has been adopted as a standard and a comparison has been made between the observed latitudes and the Mauna Kea standard. This brings out some interesting deflections of the plumb-line. See Fig. 1.

1.



Scale 100 miles to one inch.

There appears to be a disturbance of more than a minute in the direction of gravity at the south point of Hawaii (Ka Lae). At Kohala the plumb-line is deflected half a minute towards the south and at Kalaieha nearly as much towards the north, the disturbance being in both cases towards the mountain. The enormous deflection at Ka Lae (67″) is also to the northward. This is evidently caused by the great mass of Mauna Loa,* which adds its effect to that of Mauna Kea and moreover is comparatively near to the astronomical station.

On Maui the same phenomenon appears. At Haiku there is a deflection towards the south and at Kaupo there is one to the north, and as before the astronomical latitude determined

* Both Mauna Kea and Mauna Loa are nearly 14,000 feet high.

on the top of Haleakala (10,000 feet elevation) at Pakaoao appears to be a normal one for that island. Judging by analogy there seems to be no reasonable doubt that Oahu would have shown the same thing had a station been made on the summit between Kahuku and Puuloa.

The mean deflection for each of the islands (leaving out Ka Lae on the island of Hawaii) is:—

Hawaii	27″
Maui	29
Oahu	26

When we come to compare the mean latitudes for each island with one another we find that Maui is too small whether judged by the Hawaii or the Oahu standard. The amount is nearly the same in either case so that the most probable assumption is that there is a preponderance of matter deflecting the plumb-line to the northward at all the Maui stations. This supposition has been made by Professor Alexander and seems to be the most rational interpretation of the results. The following table has been furnished by him and is inserted here with his permission.

Hawaiian Latitudes.

Stations.	Astronomical.	Geodetic.	Diff.	Mauna Kea Standard.	Diff.
Oahu					
Kahuku	21° 43′ 06″·1	21° 42′ 16″·1	+50″·0	21° 42′ 43″·2	+22″·9
Puuloa	21 19 15 ·6	21 19 11 ·8	+ 3 ·8	21 19 38 ·9	−23 ·3
Honolulu	21 18 02 ·5	21 18 02 ·3	+ 0 ·2	21 18 29 ·6	−27 ·1
Waikiki	21 16 24 ·5	21 16 26 ·8	− 2 ·3	21 16 53 ·9	−29 ·4
Maui					
Lahaina	20 52 22 ·8	20 52 53 ·2	−30 ·4	20 52 34 ·5	−11 ·7
Haiku	20 56 02 ·6	20 56 04 ·0	− 1 ·4	20 55 45 ·3	+17 ·3
Pakaoao	20 42 51 ·0	20 43 21 ·6	−30 ·6	20 43 02 ·9	−11 ·9
Kaupo	20 36 40 ·8	20 37 41 ·0	−60 ·2	20 37 22 ·3	−41 ·5
Hana	20 45 38 ·9	20 45 47 ·5	− 8 ·6	20 45 28 ·8	+10 ·1
Hawaii					
Kohala	20 15 29 ·3	20 15 17 ·7	+11 ·6	20 14 59 ·0	+30 ·3
Kawaihae	20 02 05 ·9	20 02 25 ·1	−19 ·2	20 02 06 ·4	−00 ·5
Mauna Kea	19 48 52 ·0	19 49 10 ·7	−18 ·7	19 48 52 ·0	±00 ·0
Kalaieha	19 42 02 ·6	19 42 33 ·5	−30 ·9	19 42 14 ·8	−12 ·2
Hilo	19 43 11 ·2	19 43 30 ·4	−19 ·2	19 43 11 7	−00 ·5
Kailua	19 38 20 ·9	19 39 03 ·8	−42 ·9	19 38 45 ·1	−24 ·2
Ka Lae	18 53 51 ·7	18 55 17 ·7	−86 ·0	18 54 59 ·0	−67 ·3

The astronomical latitudes were determined by myself in 1883, 1887 and 1891–'92, using the method of equal zenith distances. The average probable error of a result for each station was ±0″·10. For the details of this work see Appendix, No. 14, U. S. Coast and Geodetic Survey Report for 1888. Determinations of Latitude and Gravity for the Hawaiian Government.

ART. XXIII.—*On the Glacial Lake St. Lawrence of Professor Warren Upham;* by ROBERT CHALMERS, of the Geological Survey of Canada.

IN an article in this Journal for January, 1895, entitled "Late Glacial or Champlain Subsidence and Re-elevation of the St. Lawrence River Basin," Mr. Warren Upham continues his discussions respecting hypothetical glacial lakes and glacial dams, and in endeavoring to account for the raised beaches in the region of the Great Lakes, etc., postulates still another glacial lake in the St. Lawrence valley between Lake Ontario and Quebec, held in by a glacial dam at or near the latter place. To this sheet of water he gives the name of the St. Lawrence Lake. Permit me to offer a few facts and inferences touching the question of this ice-dam and lake.

(1.) There is no evidence of a thick mass of ice having occupied the St. Lawrence valley at Quebec in the Pleistocene period. For the last ten years the writer has, at intervals, been investigating the glacial phenomena and the post-glacial shore lines, etc., of the south side of the St. Lawrence valley, especially between Mètis and the Chaudière river. During the past summer the work was revised and extended to the higher grounds of the Notre Dame Mountains in Quebec, and also to northern New Brunswick and northeastern Maine. The results do not afford any proofs of the movement of a great ice-sheet over this region at any time during the glacial period; on the contrary, the glacial phenomena on the slopes and higher grounds seem to be entirely due to local sheets of land-ice, of greater or less extent, moving in different directions, the course, on the slope facing the St. Lawrence, being mainly northward. In the bottom of the St. Lawrence valley, however, a northeast and southwest set of striæ occurs, which seems referable to the action of floating ice.

The theory that the later ice movements obliterated the earlier striæ does not find any support from the facts obtained on the south side of the St. Lawrence, so far as my examinations have extended. The glaciated surfaces everywhere exhibit criss-cross striæ, in fact these are the rule rather than the exception. The later sets, whether made by separate glaciers, or by succeeding portions of the same sheet conforming more closely to the minor topographical features as it decreased in thickness, show that the earlier striæ have not been effaced by later ice, except, perhaps to a very limited extent on exposed bosses.*

* In Mr. Upham's review of the third edition of "The Great Ice Age" by Prof. James Geikie (Am. Geologist, Jan., 1895, p. 52), he states that "the northward

The glaciation of the southern flank of the Laurentide Range on the north side of the St. Lawrence river at Quebec seems to be of much the same character as that of the south side of the river. Mr. A. P. Low, of this survey, who has examined this district in some detail, gives a list of striæ in the Annual Report of the Geological Survey of Canada, vol. v, page 48L, from which it appears that the ice movements were quite divergent in that particular locality. The Laurentide ice-sheet does not seem to have descended into the St. Lawrence valley there, unless as broken, detached glaciers. The smaller river valleys and the slopes have also influenced the ice-flow on the north side of the St. Lawrence as well as on the south side. Some of the narrow valleys between the ridges which trend along the foot hills, and are parallel thereto, have caused local glaciers to move northeastwardly in certain places, in others southwestwardly. No single dominant course was observed.

(2.) In Mr. Upham's map (Plate I) he gives the direction of the striæ on the south side of the St. Lawrence below Quebec as northeastward. Has he examined this region himself? If not, on whose authority has he reversed the courses of the striæ there, these being shown on Sir Wm. Dawson's map (The Canadian Ice Age, page 150) as pointing southwestward, and are supposed to have been produced by floating ice moving up the valley? The author's information in regard to these striæ, from whatever source it may have been obtained, is incorrect. No general sheet of land ice flowed to the northeastward in that part of the St. Lawrence valley. All the striæ *in the bottom of the valley* trending northeast and southwest are regarded as due to floating ice, and were produced in the last stage of the glacial period when the land stood at a lower level. In a few instances the southwest sides of the bosses are stossed by this floating ice as it moved down stream but the principal movement was up stream. This system of striation is traceable along the St. Lawrence valley from Metis, or lower down, westward to Montreal.

(3.) No lacustrine deposits have been found anywhere in the St. Lawrence valley beneath the Leda clay, as far as investigations have been made.

glacial flow from northern New England towards the St. Lawrence, as suggested by Chalmers, appears to have belonged only to a very late stage when the melting of the ice in the St. Lawrence valley, proceeding faster than on the mountainous area at the south, left there a large isolated remnant of the departing ice-sheet." I have nowhere stated that I regard the northward ice-flow referred to as belonging to a very late stage of the glacial period; on the contrary, I hold that wherever the northward ice-movements occurred they belong to the maximum stage of the ice age as well as to the melting or later stage; but my own observations have not extended further west than Lake Megantic.

All the facts taken together, therefore, show that the hypothesis of an ice dam at Quebec holding in a lake in the St. Lawrence valley between that point and Lake Ontario, as set forth by Mr. Upham, is untenable.

The glaciation of the St. Lawrence valley is exceedingly complex, and cannot be explained by *à priori* theories. The problems it presents must be solved by actual field investigations. The region is a most interesting one, however, and I invite glacialists to come and see the facts for themselves before propounding any grand generalizations respecting its Pleistocene geology.

Ottawa, Jan. 16, 1895.

Art. XXIV.—*Argon, a New Constituent of the Atmosphere;* by Lord Rayleigh and Professor William Ramsay.

[Abstract of a paper read before the Royal Society; from advance sheets sent to this Journal by the authors.]

I. *Density of Nitrogen from Various Sources.*

In a former paper* it has been shown that nitrogen extracted from chemical compounds is about $\frac{1}{2}$ per cent lighter than "atmospheric nitrogen."

The mean numbers for the weights of gas contained in the globe used were as follows:—

From nitric oxide	2·3001
From nitrous oxide	2·2990
From ammonium nitrite	2·2987

while for "atmospheric nitrogen" there was found—

By hot copper, 1892	2·3103
By hot iron, 1893	2·3100
By ferrous hydrate, 1894	2·3102

At the suggestion of Professor Thorpe experiments were subsequently tried with nitrogen liberated from *urea* by the action of sodium hypobromite. The hypobromite was prepared from commercial materials in the proportions recommended for the analysis of urea. The reaction was well under control, and the gas could be liberated as slowly as desired.

In the first experiment the gas was submitted to no other treatment than slow passage through potash and phosphoric anhydride, but it soon became apparent that the nitrogen was

* Rayleigh, On an Anomaly encountered in Determinations of the Density of Nitrogen Gas, Proc. Roy. Soc., vol. lv, p. 340, 1894.

contaminated. The "inert and inodorous" gas attacked vigorously the mercury of the Töpler pump, and was described as smelling like a dead rat. As to the weight, it proved to be in excess even of the weight of atmospheric nitrogen.

The corrosion of the mercury and the evil smell were in great degree obviated by passing the gas over hot metals. For the fillings of June 6, 9 and 13 the gas passed through a short length of tube containing copper in the form of fine wire heated by a flat Bunsen burner, then through the furnace over red-hot iron, and back over copper oxide. On June 19 the furnace tubes were omitted, the gas being treated with the red-hot copper only. The mean result, reduced so as to correspond with those above quoted, is 2·2985.

Without using heat, it has not been found possible to prevent the corrosion of the mercury. Even when no urea is employed, and air simply bubbled through, the hypobromite solution is allowed to pass with constant shaking over mercury contained in a U-tube, the surface of the metal was soon fouled.

Although the results relating to urea nitrogen are interesting for comparison with that obtained from other nitrogen compounds, the original object was not attained on account of the necessity of retaining the treatment with hot metals. We have found, however, that nitrogen from ammonium nitrite may be prepared, without the employment of hot tubes, whose weight agrees with that above quoted. It is true that the gas smells slightly of ammonia, easily removable by sulphuric acid, and apparently also of oxides of nitrogen. The mean result from three fillings is 2·2987.

It will be seen that, in spite of the slight nitrous smell, there is no appreciable difference in the densities of gas prepared from ammonium nitrite with and without the treatment by hot metals. The result is interesting as showing that the agreement of numbers obtained for chemical nitrogen does not depend upon the use of a red heat in the process of purification.

The five results obtained in more or less distinct ways for chemical nitrogen stand thus:—

From nitric oxide	2·3001
From nitrous oxide	2·2990
From ammonium nitrite purified at a red heat	2·2987
From urea	2·2985
From ammonium nitrite purified in the cold	2·2987
Mean	2·2990

These numbers, as well as those above quoted for "atmospheric nitrogen," are subject to a deduction of 0·0006 for the shrinkage of the globe when exhausted.* If they are then multiplied in the ratio of 2·3108 : 1·2572, they will express the weights of the gas in grams per liter. Thus, as regards the mean numbers, we find as the weight per liter under standard conditions of chemical nitrogen 1·2505, that of atmospheric nitrogen being 1·2572.

It is of interest to compare the density of nitrogen obtained from chemical compounds with that of oxygen. We have $N_2 : O_2 = 2·2984 : 2·6276 = 0·87471$; so that if $O_2 = 16$, $N_2 = 13·9954$. Thus, when the comparison is with chemical nitrogen, the ratio is very nearly that of 16 : 14; but if "atmospheric nitrogen" be substituted, the ratio of small integers is widely departed from.

To the above list may be added nitrogen prepared in yet another manner, whose weight has been determined subsequently to the isolation of the new dense constituent of the atmosphere. In this case nitrogen was actually extracted from air by means of magnesium. The nitrogen thus separated was then converted into ammonia by action of water upon the magnesium nitride and afterwards liberated in the free state by means of calcium hypochlorite. The purification was conducted in the usual way, and included passage over red-hot copper and copper oxide. The following was the result:

Globe empty, Oct. 30, Nov. 5	2·82313
Globe full, Oct. 31	0·52395
Weight of gas	2·29918

It differs inappreciably from the mean of other results, viz: 2·2990, and is of special interest as relating to gas which at one stage of its history formed part of the atmosphere.

Another determination, with a different apparatus, of the density of "chemical" nitrogen from the same source, magnesium nitride, which had been prepared by passing "atmospheric" nitrogen over ignited magnesium, may here be recorded. The sample differed from that previously mentioned, inasmuch as it had not been subjected to treatment with red-hot copper. After treating the nitride with water, the resulting ammonia was distilled off and collected in hydrochloric acid; the solution was evaporated by degrees, the dry ammonium chloride was dissolved in water, and its concentrated solution added to a freshly-prepared solution of sodium hypobromite. The nitrogen was collected in a gas-holder over water which

* Rayleigh, On the Densities of the Principal Gases, Proc. Roy. Soc., vol. liii, p. 134, 1893.

had previously been boiled, so as, at all events partially to expel air. The nitrogen passed into the vacuous globe through a solution of potassium hydroxide, and through two drying-tubes, one containing soda-lime, and the other phosphoric anhydride.

At 18·38° C. and 754·4^{mm} pressure, 162·843^{cc} of this nitrogen weighed 0·18963 gram. Hence,

Weight of 1 liter at 0° C. and 760^{mm} pressure = 1·2521 gram.

The mean result of the weight of 1 liter of "chemical" nitrogen has been found to equal 1·2505. It is therefore seen that "chemical" nitrogen, derived from "atmospheric" nitrogen, without any exposure to red-hot copper, possesses the usual density.

Experiments were also made, which had for their object to prove that the ammonia produced from the magnesium nitride is identical with ordinary ammonia, and contains no other compound of a basic character. For this purpose the ammonia was converted into ammonium chloride, and the percentage of chlorine determined by titration with a solution of silver nitrate which had been standardized by titrating a specimen of pure sublimed ammonium chloride. The silver solution was of such a strength that 1^{cc} precipitated the chlorine from 0·001701 gram of ammonium chloride.

1. Ammonium chloride from orange-colored sample of magnesium nitride contained 66·35 per cent of chlorine.
2. Ammonium chloride from blackish magnesium nitride contained 66·35 per cent of chlorine.
3. Ammonium chloride from nitride containing a large amount of unattacked magnesium contained 66·30 per cent of chlorine.

Taking for the atomic weights of hydrogen H = 1·0032, of nitrogen N = 14·04, and of chlorine Cl = 35·46, the theoretical amount of chlorine in ammonium chloride is 66·27 per cent.

From these results—that nitrogen prepared from magnesium nitride, obtained by passing "atmospheric" nitrogen over red-hot magnesium has the density of "chemical" nitrogen, and that ammonium chloride, prepared from magnesium nitride, contains practically the same percentage of chlorine as pure ammonium chloride—it may be concluded that red-hot magnesium withdraws from "atmospheric nitrogen" no substance other than nitrogen capable of forming a basic compound with hydrogen.

II. *Reasons for suspecting a hitherto Undiscovered Constituent in Air.*

When the discrepancy of weights was first encountered, attempts were naturally made to explain it by contamination with known impurities. Of these the most likely appeared to be hydrogen, present in the lighter gas in spite of the passage over red-hot cupric oxide. But inasmuch as the intentional introduction of hydrogen into the heavier gas, afterwards treated in the same way with cupric oxide, had no effect upon its weight, this explanation had to be abandoned, and finally it became clear that the difference could not be accounted for by the presence of any known impurity. At this stage it seemed not improbable that the lightness of the gas extracted from chemical compounds was to be explained by partial dissociation of nitrogen molecules N_2 into detached atoms. In order to test this suggestion both kinds of gas were submitted to the action of the silent electric discharge, with the result that both retained their weights unaltered. This was discouraging, and a further experiment pointed still more markedly in the negative direction. The chemical behavior of nitrogen is such as to suggest that dissociated atoms would possess a high degree of activity, and that even though they might be formed in the first instance their life would probably be short. On standing they might be expected to disappear, in partial analogy with the known behavior of ozone. With this idea in view, a sample of chemically prepared nitrogen was stored for eight months. But at the end of this time the density showed no sign of increase, remaining exactly as at first.*

Regarding it as established that one or other of the gases must be a mixture, containing, as the case might be, an ingredient much heavier or much lighter than ordinary nitrogen, we had to consider the relative probabilities of the various possible interpretations. Except upon the already discredited hypothesis of dissociation, it was difficult to see how the gas of chemical origin could be a mixture. To suppose this would be to admit two kinds of nitric acid, hardly reconcilable with the work of Stas and others upon the atomic weight of that substance. The simplest explanation in many respects was to admit the existence of a second ingredient in air from which oxygen, moisture, and carbonic anhydride had already been removed. The proportional amount required was not great. If the density of the supposed gas were double that of nitrogen $\frac{1}{2}$ per cent only by volume would be needed; or if the density were but half as much again as that of nitrogen, then 1 per cent would still suffice. But in accepting this explana-

* Proc. Roy. Soc., vol. lv, p. 344, 1894.

tion, even provisionally, we had to face the improbability that a gas surrounding us on all sides, and present in enormous quantities, could have remained so long unsuspected.

The method of most universal application by which to test whether a gas is pure or a mixture of components of different densities is that of diffusion. By this means Graham succeeded in effecting a partial separation of the nitrogen and oxygen of the air, in spite of the comparatively small difference of densities. If the atmosphere contain an unknown gas of anything like the density supposed, it should be possible to prove the fact by operations conducted upon air which had undergone atmolysis. This experiment, although in view from the first, was not executed until a later stage of the inquiry (§ 6), when results were obtained sufficient of themselves to prove that the atmosphere contains a previously unknown gas.

But although the method of diffusion was capable of deciding the main, or at any rate the first question, it held out no prospect of isolating the new constituent of the atmosphere, and we, therefore, turned our attention in the first instance to the consideration of methods more strictly chemical. And here the question forced itself upon us as to what really was the evidence in favor of the prevalent doctrine that the inert residue from air after withdrawal of oxygen, water, and carbonic anhydride, is all of one kind.

The identification of "phlogisticated air" with the constituent of nitric acid is due to Cavendish, whose method consisted in operating with electric sparks upon a short column of gas confined with potash over mercury at the upper end of an inverted U-tube.*

Attempts to repeat Cavendish's experiment in Cavendish's manner have only increased the admiration with which we regard this wonderful investigation. Working on almost microscopical quantities of material, and by operations extending over days and weeks, he thus established one of the most important facts in chemistry. And what is still more to the purpose, he raises as distinctly as we could do, and to a certain extent resolves, the question above suggested. The passage is so important that it will be desirable to quote it at full length.

"As far as the experiments hitherto published extend, we scarcely know more of the phlogisticated part of our atmosphere, than that it is not diminished by lime-water, caustic alkalies, or nitrous air; that it is unfit to support fire, or maintain life in animals; and that its specific gravity is not much less than that of common air: so that though the nitrous acid,

* Experiments on Air, Phil. Trans., vol. lxxv. p. 372, 1785.

by being united to phlogiston, is converted into air possessed of these properties, and consequently, though it was reasonable to suppose, that part at least of the phlogisticated air of the atmosphere consists of this acid united to phlogiston, yet it was fairly to be doubted whether the whole is of this kind, or whether there are not in reality many different substances compounded together by us under the name of phlogisticated air. I therefore made an experiment to determine whether the whole of a given portion of the phlogisticated air of the atmosphere could be reduced to nitrous acid, or whether there was not a part of a different nature to the rest, which would refuse to undergo that change. The foregoing experiments indeed in some measure decided this point, as much the greatest part of the air let up into the tube lost its elasticity; yet as some remained unabsorbed it did not appear for certain whether that was of the same nature as the rest or not. For this purpose I diminished a similar mixture of dephlogisticated and common air, in the same manner as before, till it was reduced to a small part of its original bulk. I then, in order to decompound as much as I could of the phlogisticated air which remained in the tube, added some dephlogisticated air to it, and continued the spark until no further diminution took place. Having by these means condensed as much as I could of the phlogisticated air, I let up some solution of liver of sulphur to absorb the dephlogisticated air; after which only a small bubble of air remained unabsorbed, which certainly was not more than $\frac{1}{120}$th of the bulk of the phlogisticated air let up into the tube; so that if there is any part of the phlogisticated air of our atmosphere which differs from the rest, and cannot be reduced to nitrous acid, we may safely conclude that it is not more than $\frac{1}{120}$th part of the whole."

Although Cavendish was satisfied with his result, and does not decide whether the small residue was genuine, our experiments about to be related render it not improbable that his residue was really of a different kind from the main bulk of the "phlogisticated air," and contained the gas now called argon.

Cavendish gives data* from which it is possible to determine the rate of absorption of the mixed gases in his experiment. This was about 1^{cc} per hour, of which two-fifths would be nitrogen.

III. *Methods of causing Free Nitrogen to combine.*

To eliminate nitrogen from air, in order to ascertain whether any other gas could be detected, involves the use of some

* Phil. Trans., vol. lxxviii, p. 271, 1788.

absorbent. The elements which have been found to combine directly with nitrogen are: boron, silicon, titanium, lithium, strontium, barium, magnesium, aluminium, mercury, and, under the influence of an electric discharge, hydrogen in presence of acid, and oxygen in presence of alkali. Besides these, a mixture of barium carbonate and carbon at a high temperature is known to be effective. Of those tried, magnesium in the form of turnings was found to be the best. When nitrogen is passed over magnesium, heated in a tube of hard glass to bright redness, combustion with incandescence begins at the end of the tube through which the gas is introduced, and proceeds regularly until all the metal has been converted into nitride. Between 7 and 8 liters of nitrogen can be absorbed in a single tube; the nitride formed is a porous, dirty, orange-colored substance.

IV. *Early Experiments on sparking Nitrogen with Oxygen in presence of Alkali.*

In our earliest attempts to isolate the suspected gas by the method of Cavendish, we used a Ruhmkorff coil of medium size actuated by a battery of five Grove cells. The gases were contained in a test-tube standing over a large quantity of weak alkali, and the current was conveyed in wires insulated by U-shaped glass tubes passing through the liquid round the mouth of the test-tube. With the given battery and coil a somewhat short spark or arc of about 5^{mm} was found to be more favorable than a longer one. When the mixed gases were in the right proportion the rate of absorption was about 30^{cc} per hour, or thirty times as fast as Cavendish could work with the electrical machine of his day.

To take an example, one experiment of this kind started with 50^{cc} of air. To this oxygen was gradually added until, oxygen being in excess, there was no perceptible contraction during an hour's sparking. The remaining gas was then transferred at the pneumatic trough to a small measuring vessel, sealed by mercury, in which the volume was found to be $1{\cdot}0^{cc}$. On treatment with alkaline pyrogallate, the gas shrank to $0{\cdot}32^{cc}$. That this small residue could not be nitrogen was argued from the fact that it had withstood the prolonged action of the spark, although mixed with oxygen in nearly the most favorable proportion.

The residue was then transferred to the test-tube with an addition of another 50^{cc} of air, and the whole worked up with oxygen as before. The residue was now $2{\cdot}2^{cc}$, and, after removal of oxygen, $0{\cdot}76^{cc}$.

Although it seemed almost impossible that these residues could be either nitrogen or hydrogen, some anxiety was not unnatural, seeing that the final sparking took place under somewhat abnormal conditions. The space was very restricted, and the temperature (and with it the proportion of aqueous vapor) was unduly high. But any doubts that were felt upon this score were removed by comparison experiments in which the whole quantity of air operated on was very small. Thus, when a mixture of 5^{cc} of air with 7^{cc} of oxygen was sparked for 1¼ hours, the residue was 0·47^{cc}, and after removal of oxygen 0·06^{cc}. Several repetitions having given similar results, it became clear that the final residue did not depend upon anything that might happen when sparks passed through a greatly reduced volume, *but was in proportion to the amount of air operated upon.*

No satisfactory examination of the residue which refused to be oxidized could be made without the accumulation of a larger quantity. This, however, was difficult of attainment at the time in question. It was thought that the cause probably lay in the solubility of the gas in water, a suspicion since confirmed. At length, however, a sufficiency was collected to allow of sparking in a specially constructed tube, when a comparison with the air spectrum, taken under similar conditions, proved that, at any rate, the gas was not nitrogen. At first scarcely a trace of the principal nitrogen lines could be seen, but after standing over water for an hour or two these lines became apparent.

V. *Early experiments on withdrawal of Nitrogen from Air by means of red-hot Magnesium.*

A preliminary experiment carried out by Mr. Percy Williams on the absorption of atmospheric nitrogen, freed from oxygen by means of red-hot copper, in which the gas was not passed over, but simply allowed to remain in contact with the metal, gave a residue of density 14·88. This result, although not conclusive, was encouraging; and an attempt was made, on a larger scale, by passing atmospheric nitrogen backwards and forwards over red-hot magnesium from one large gas-holder to another to obtain a considerable quantity of the heavier gas. In the course of ten days, about 1500^{cc} were collected and transferred gradually to a mercury gas-holder, from which the gas was passed over soda-lime, phosphoric anhydride, magnesium at a red heat, copper oxide, soda-lime, and phosphoric anhydride into a second mercury gas-holder. After some days the gas was reduced in volume to about 200^{cc}, and its density was found to be 16·1. After further absorption, in which the

volume was still further reduced, the density of the residue was increased to 19·09.

On passing sparks for several hours through a mixture of a small quantity of this gas with oxygen, its volume was still further reduced. Assuming that this reduction was due to the further elimination of nitrogen, the density of the remaining gas was calculated to be 20·0.

The spectrum of the gas of density 19·09, though showing nitrogen bands, showed many other lines which were not recognizable as belonging to any known element.

VI. *Proof of the presence of Argon in Air by means of Atmolysis.*

It has already (§ 2) been suggested that if "atmospheric nitrogen" contains two gases of different densities, it should be possible to obtain direct evidence of the fact by the method of atmolysis. The present section contains an account of carefully conducted experiments directed to this end.

The atmolyser was prepared (after Graham) by combining a number of "churchwarden" tobacco pipes. At first twelve pipes were used in three groups, each group including four pipes connected in series. The three groups were then connected in parallel, and placed in a large glass tube closed in such a way that a partial vacuum could be maintained in the space outside the pipes by a water pump. One end of the combination of pipes was open to the atmosphere; the other end was connected to a bottle aspirator, initially full of water, and so arranged as to draw about 2 per cent of the air which entered the other end of the pipes. The gas collected was thus a very small proportion of that which leaked through the pores of the pipes, and should be relatively rich in the heavier constituents of the atmosphere. The flow of water from the aspirator could not be maintained very constant, but the rate of 2 per cent was never much exceeded.

The air thus obtained was treated exactly as ordinary air had been treated in determinations of the density of atmospheric nitrogen. Oxygen was removed by red-hot copper, followed by cupric oxide, ammonia by sulphuric acid, moisture and carbonic acid by potash and phosphoric anhydride.

In a total weight of approximately 2·3 grams the excess of weight of the diffused nitrogen over ordinary atmospheric nitrogen was in four experiments, 0·0049, 0·0014, 0·0027, 0·0015.

The mean excess of the four determinations is 0·00262 gram, or, if we omit the first, which depended upon a vacuum weighing of two months old, 0·00187 gram.

The gas from prepared air was thus in every case denser than from unprepared air, and to an extent much beyond the possible errors of experiment. The excess was, however, less than had been expected, and it was thought that the arrangement of the pipes could be improved. The final delivery of gas from each of the groups in parallel being so small in comparison with the whole streams concerned, it seemed possible that each group was not contributing its proper share, and even that there might be a flow in the wrong direction at the delivery end of one or two of them. To meet this objection, the arrangement in parallel had to be abandoned, and for the remaining experiments eight pipes were connected in simple series. The porous surface in operation was thus reduced, but this was partly compensated for by an improved vacuum. Two experiments were made under the new conditions, in which the excess was I, 0·0037; II, 0·0033.

The excess being larger than before is doubtless due to the greater efficiency of the atmolysing apparatus. It should be mentioned that the above recorded experiments include all that have been tried, and the conclusion seems inevitable that "atmospheric nitrogen" is a mixture, and not a simple body.

It was hoped that the concentration of the heavier constituent would be sufficient to facilitate its preparation in a pure state by the use of prepared air in substitution for ordinary air in the oxygen apparatus. The advance of $3\frac{1}{2}$ milligrams on the 11 milligrams, by which atmospheric nitrogen is heavier than chemical nitrogen, is indeed not to be despised, and the use of prepared air would be convenient if the diffusion apparatus could be set up on a large scale and be made thoroughly self-acting.

VII. *Negative Experiments to prove that Argon is not derived from Nitrogen from Chemical Sources.*

Although the evidence of the existence of argon in the atmosphere, derived from the comparison of densities of atmospheric and chemical nitrogen and from the diffusion experiments (§ VI), appeared overwhelming, we have thought it undesirable to shrink from any labor that would tend to complete the verification. With this object in view, an experiment was undertaken and carried to a conclusion on November 13, in which 3 liters of chemical nitrogen, prepared from ammonium nitrite, were treated with oxygen in precisely the manner in which atmospheric nitrogen had been found to yield a residue of argon. The gas remaining at the close of the large scale operations was worked up as usual with battery and coil until the spectrum showed only slight traces of the nitrogen lines. When cold, the residue measured 4^{cc}. This

was transferred, and after treatment with alkaline pyrogallate to remove oxygen measured 3·3cc. If atmospheric nitrogen had been employed, the final residue should have been about 30cc. Of the 3·3cc actually left, a part is accounted for by an accident, and the result of the experiment is to show that argon is not formed by sparking a mixture of oxygen and chemical nitrogen.

In a second experiment of the same kind 5660cc of nitrogen from ammonium nitrite was treated with oxygen. The final residue was 3·5cc, and was found to consist mainly of argon.

The source of the residual argon is to be sought in the water used for the manipulation of the large quantities of gas (6 liters of nitrogen and 11 liters of oxygen) employed. When carbonic acid was collected in a similar manner and subsequently absorbed by potash, it was found to have acquired a contamination consistent with this explanation.

Negative experiments were also carried out, absorbing nitrogen by means of magnesium. In one instance 3 liters of nitrogen prepared from ammonium chloride and bleaching-powder was reduced in volume to 4·5cc, and on sparking with oxygen its volume was further reduced to about 3cc. The residue appeared to consist of argon. Another experiment, in which 15 liters of nitrogen from ammonium nitrite was absorbed, gave a final residue of 3·5cc. Atmospheric nitrogen, in the latter case, would have yielded 150cc, hence less than $\frac{1}{40}$th of the normal quantity was obtained. It should be mentioned that leakage occurred at one stage, by which perhaps 200cc of air entered the apparatus; and, besides, the nitrogen was collected over water from which it doubtless acquired some argon. Quantitative negative experiments of this nature are exceedingly difficult, and require a long time to carry them to a successful conclusion.

VIII. *Separation of Argon on a Large Scale.*

To prepare argon on a large scale, air is freed from oxygen by means of red-hot copper. The residue is then passed from a gas-holder through a combustion-tube, heated in a furnace, and containing copper, in order to remove all traces of oxygen; the issuing gas is then dried by passage over soda-lime and phosphorus pentoxide, after passage through a small U-tube containing sulphuric acid, to indicate the rate of flow. It then enters a combustion-tube packed tightly with magnesium turnings, and heated to redness in a second furnace. From this tube it passes through a second index-tube, and enters a small gas-holder capable of containing 3 or 4 liters. A single tube of magnesium will absorb from 7 to 8 liters of nitrogen.

The temperature must be nearly that of the fusion of the glass, and the current of gas must be carefully regulated, else the heat developed by the union of the magnesium with nitrogen will fuse the tube.

Having collected the residue from 100 or 150 liters of atmospheric nitrogen, which may amount to 4 or 5 liters, it is transferred to a small gas-holder connected with an apparatus, whereby, by means of a species of a self-acting Sprengel's pump, the gas is caused to circulate through a tube half filled with copper and half with copper oxide; it then traverses a tube half filled with soda-lime and half with phosphorus pentoxide; it then passes a reservoir of about 300^{cc} capacity from which, by raising a mercury reservoir, it can be expelled into a small gas-holder. Next it passes through a tube containing magnesium turnings heated to bright redness. The gas is thus freed from any possible contamination with oxygen, hydrogen, or hydrocarbons, and nitrogen is gradually absorbed. As the amount of gas in the tubes and reservoir diminishes in volume, it draws supplies from the gas-holder, and, finally, the circulating system is full of argon in a pure state. The circulating system of tubes is connected with a mercury pump, so that, in changing the magnesium tube, no gas may be lost. Before ceasing to heat the magnesium tube the system is pumped empty, and the collected gas is restored to the gas-holder; finally, all the argon is transferred from the mercury reservoir to the second small gas-holder, which should preferably be filled with water saturated with argon, so as to prevent contamination from oxygen or nitrogen; or, if preferred, a mercury gas-holder may be employed. The complete removal of nitrogen from argon is very slow towards the end, but circulation for a couple of days usually effects it.

The principal objection to the oxygen method of isolating argon, as hitherto described, is the extreme slowness of the operation. In extending the scale we had the great advantage of the advice of Mr. Crookes, who not long since called attention to the flame rising from platinum terminals, which convey a high tension alternating electric discharge, and pointed out its dependence upon combustion of the nitrogen and oxygen of the air.* The plant consists of a De Meritens alternator, actuated by a gas engine, and the currents are transformed to a high potential by means of a Ruhmkorff or other suitable induction coil. The highest rate of absorption of the mixed gases yet attained is 3 liters per hour, about 3000 times that of Cavendish. It is necessary to keep the apparatus cool, and from this and other causes a good many difficulties have been encountered.

* Chemical News, vol. lxv, p. 301, 1892.

In one experiment of this kind, the total air led in after seven days' working, amounted to 7925^{cc}, and of oxygen (prepared from chlorate of potash), 9137^{cc}. On the eighth and ninth days oxygen alone was added, of which about 500^{cc} was consumed, while there remained about 700^{cc} in the flask. Hence the proportion in which the air and oxygen combined was as 79 : 96. The progress of the removal of the nitrogen was examined from time to time with the spectroscope, and became ultimately very slow. At last the yellow line disappeared, the contraction having apparently stopped for two hours. It is worthy of notice that with the removal of the nitrogen, the arc discharge changes greatly in appearance, becoming narrower and blue rather than greenish in color.

The final treatment of the residual 700^{cc} of gas was on the model of the small scale operations already described. Oxygen or hydrogen could be supplied at pleasure from an electrolytic apparatus, but in no way could the volume be reduced below 65^{cc}. This residue refused oxidation, and showed no trace of the yellow line of nitrogen, even under favorable conditions.

When the gas stood for some days over water, the nitrogen line reasserted itself in the spectrum, and many hours' sparking with a little oxygen was required again to get rid of it. Intentional additions of air to gas free from nitrogen showed that about $1\frac{1}{2}$ per cent was clearly, and about 3 per cent was conspicuously, visible. About the same numbers apply to the visibility of nitrogen in oxygen when sparked under these conditions, that is, at atmospheric pressure, and with a jar connected to the secondary terminals.

IX. *Density of Argon prepared by means of Oxygen.*

A first estimate of the density of argon prepared by the oxygen method was founded upon the data already recorded respecting the volume present in air, on the assumption that the accurately known densities of atmospheric and of chemical nitrogen differ on account of the presence of argon in the former, and that during the treatment with oxygen nothing is oxidized except nitrogen. Thus if

D = density of chemical nitrogen,
D' = " atmospheric nitrogen,
d = " argon,
α = proportional volume of argon in atmospheric nitrogen,

the law of mixtures give

$$\alpha d + (1-\alpha)\mathrm{D} = \mathrm{D}',$$

or

$$d = \mathrm{D} + (\mathrm{D}'-\mathrm{D})/\alpha.$$

In this formula $D'-D$ and a are both small, but they are known with fair accuracy. From the data already given

$$a = \frac{65}{0{\cdot}79 \times 7925},$$

whence if (on an arbitrary scale of reckoning) $D = 2{\cdot}2990$, $D' = 2{\cdot}3102$, we find $d = 3{\cdot}378$. Thus if N_2 be 14, or O_2 be 16, the density of argon is 20·6.

A direct determination by weighing is desirable, but hitherto it has not been feasible to collect by this means sufficient to fill the large globe employed for other gases. A *mixture* of about 400^{cc} of argon with pure oxygen, however, gave the weight 2·7315, 0·1045 in excess of the weight of oxygen, viz: 2·6270. Thus, if a be the ratio of the volume of argon to the whole volume, the number for argon will be

$$2{\cdot}6270 + 0{\cdot}1045\,/\,a$$

The value of a, being involved only in the excess of weight above that of oxygen, does not require to be known very accurately. Sufficiently concordant analyses by two methods gave $a = 0{\cdot}1845$; whence for the weight of the gas we get 3·193, so that, if $O_2 = 16$, the density of the gas would be 19·45. An allowance for residual nitrogen, still visible in the gas before admixture of oxygen, raises this number to 19·7, which may be taken as the density of pure argon resulting from this determination.

X. *Density of Argon prepared by means of Magnesium.*

The density of the original sample of argon prepared has already been mentioned. It was 19·09; and, after sparking with oxygen, it was calculated to be 20·0. The most reliable results of a number of determinations give it as 19·90. The difficulty in accurately determining the density is to make sure that all nitrogen has been removed. The sample of density 19·90 showed no spectrum of nitrogen when examined in a vacuum tube. It is right, however, to remark that the highest density registered was 20·38. But there is some reason here to distrust the weighing of the vacuous globe.

XI. *Spectrum of Argon.*

The spectrum of argon, seen in a vacuum tube of about 3^{mm} pressure, consists of a great number of lines, distributed over almost the whole visible field. Two lines are specially characteristic; they are less refrangible than the red lines of hydrogen or lithium, and serve well to identify the gas, when examined in this way. Mr. Crookes, who will give a full

account of the spectrum in a separate communication, has kindly furnished us with the accurate wave-lengths of these lines, as well as of some others next to be described; they are respectively 696·56 and 705·64, 10^{-6} mm.

Besides these red lines, a bright yellow line, more refrangible than the sodium line, occurs at 603·84. A group of five bright green lines occurs next, besides a number of less intensity. Of the group of five, the second, which is perhaps the most brilliant, has the wave-length 561·00. There is next a blue or blue-violet line of wave-length 470·2; and last, in the less easily visible part of the spectrum, there are five strong violet lines, of which the fourth, which is the most brilliant, has the wave-length 420·0.

Unfortunately, the red lines, which are not to be mistaken for those of any other substance, are not easily seen when a jar discharge is passed through argon at atmospheric pressure. The spectrum seen under these conditions has been examined by Professor Schuster. The most characteristic lines are perhaps those in the neighborhood of F, and are very easily seen if there be not too much nitrogen, in spite of the presence of some oxygen and water vapor. The approximate wave-lengths are—

487·91	Strong.
[486·07]	F.
484·71	Not quite so strong.
480·52	Strong.
476·50	Fairly strong characteristic triplet.
473·53	
472·56	

It is necessary to anticipate Mr. Crookes's communication, and to state that when the current is passed from the induction coil in one direction, that end of the capillary tube next the positive pole appears of a redder, and that next the negative pole of a bluer hue. There are, in effect, two spectra, which Mr. Crookes has succeeded in separating to a considerable extent. Mr. E. C. Baly,* who has noticed a similar phenomenon, attributes it to the presence of two gases. He says:—"When an electric current is passed through a mixture of two gases, one is separated from the other and appears in the negative glow." The conclusion would follow that what we have termed "argon" is in reality a mixture of two gases which have as yet not been separated. This conclusion, if true, is of great importance, and experiments are now in progress to test it by the use of other physical methods. The full bearing of this possibility will appear later.

* Proc. Phys. Soc., 1893, p. 147.

The presence of a small quantity of nitrogen interferes greatly with the argon spectrum. But we have found that in a tube with platinum electrodes, after the discharge has been passed for four hours, the spectrum of nitrogen disappears, and the argon spectrum manifests itself in full purity. A specially constructed tube with magnesium electrodes, which we hoped would yield good results, removed all traces of nitrogen, it is true; but hydrogen was evolved from the magnesium, and showed its characteristic lines very strongly. However, these are easily identified. The gas evolved on heating magnesium *in vacuo*, as proved by a separate experiment, consists entirely of hydrogen.

Mr. Crookes has proved the identity of the chief lines of the spectrum of gas separated from air-nitrogen by aid of magnesium with that remaining after sparking the air-nitrogen with oxygen in presence of caustic soda solution.

Professor Schuster also has found the principal lines identical in the spectra of the two gases, as observed by the jar discharge at atmospheric pressure.

XII. *Solubility of Argon in Water.*

Determinations of the solubility in water of argon, prepared by sparking, gave 3·94 volumes per 100 of water at 12°. The solubility of gas prepared by means of magnesium was found to be 4·05 volumes per 100 at 13·9°. The gas is therefore about 2½ times as soluble as nitrogen, and possesses approximately the same solubility as oxygen.

The fact that argon is more soluble than nitrogen would lead us to expect it in increased proportion in the dissolved gases of rain water. Experiment has confirmed this anticipation. "Nitrogen" prepared from the dissolved gases of water supplied from a rain-water cistern was weighed upon two occasions. The weights, corresponding to those recorded in § I, were 2·3221 and 2·3227, showing an excess of 24 milligrams above the weight of true nitrogen. Since the corresponding excess for "atmospheric nitrogen" is 11 milligrams, we conclude that the water "nitrogen" is relatively more than twice as rich in argon.

On the other hand, gas evolved from the hot spring at Bath, and collected for us by Dr. A. Richardson, gave a residue after removal of oxygen and carbonic acid, whose weight was only about midway between that of true and atmospheric nitrogen.

XIII. *Behavior at Low Temperatures.**

Preliminary experiments, carried out to liquefy argon at a pressure of about 100 atmospheres, and at a temperature of

* The arrangements for the experiments upon this branch of the subject were left entirely in Professor Ramsay's hands.

—90°, failed. No appearance of liquefaction could be observed.

Professor Charles Olszewski, of Cracow, the well-known authority on the constants of liquefied gases at low temperatures, kindly offered to make experiments on the liquefaction of argon. His results are embodied in a separate communication, but it is allowable to state here that the gas has a lower critical temperature (—121°) and a lower boiling point (—187°) than oxygen, and that he has succeeded in solidifying argon to white crystals, melting at —189·6°. The density of the liquid is approximately 1·5, that of oxygen being 1·124, and of nitrogen 0·885. The sample of gas he experimented with was exceptionally pure, and had been prepared by help of magnesium. It showed no trace of nitrogen when examined in a vacuum tube.

XIV. *Ratio of Specific Heats.*

In order to decide regarding the elementary or compound nature of argon, experiments were made on the velocity of sound in it. It will be remembered that, from the velocity of sound in a gas, the ratio of specific heat at constant pressure to that at constant volume can be deduced by means of the equation

$$n\lambda = v = \sqrt{\left\{ \frac{e}{d}(1 + \alpha t)\frac{C_p}{C_v} \right\}},$$

when n is the frequency, λ the wave-length of sound, v its velocity, e the isothermal elasticity, d the density, $(1 + \alpha t)$ the temperature correction, C_p the specific heat at constant pressure, and C_v that at constant volume. In comparing two gases at the same temperature, each of which obeys Boyle's law with sufficient approximation, and in using the same sound, many of these terms disappear, and the ratio of specific heats of one gas may be deduced from that of the other, if known, by means of the proportion

$$\lambda^2 d : \lambda'^2 d' :: 1{\cdot}41 : x$$

where, for example, λ and d refer to air, of which the ratio is 1·41, according to observations by Röntgen, Wüllner, Kayser, and Jamin and Richard.

Two completely different series of observations, one in a tube of about 2mm. diameter, and one in one of 8mm., made with entirely different samples of gas, gave, the first, 1·65 as the ratio, and, the second, 1·61.

Experiments made with the first tube, to test the accuracy of its working, gave for carbon dioxide the ratio 1·276, instead

of 1·288, the mean of all previous determinations; and the half wave-length of sound in hydrogen was found to be 73·6, instead of 74·5, the mean of those previously found. The ratio of the specific heats of hydrogen found was 1·39, instead of 1·402.

There can be no doubt, therefore, that argon gives practically the ratio of specific heats, viz: 1·66, proper to a gas in which all the energy is translational. The only other gas which has been found to behave similarly is mercury gas, at a high temperature.*

XV. *Attempts to induce Chemical Combination.*

Many attempts to induce argon to combine will be described in full in the complete paper. Suffice it to say here, that all such attempts have as yet proved abortive. Argon does not combine with oxygen in presence of alkali under the influence of the electric discharge, nor with hydrogen in presence of acid or alkali also when sparked; nor with chlorine, dry or moist, when sparked; nor with phosphorus at a bright-red heat, nor with sulphur at bright redness. Tellurium may be distilled in a current of the gas; so may sodium and potassium, their metallic luster remaining unchanged. It is unabsorbed by passing it over fused red-hot caustic soda, or soda-lime heated to bright redness; it passes unaffected over fused and bright red-hot potassium nitrate; and red-hot sodium peroxide does not combine with it. Persulphides of sodium and calcium are also without action at a red heat. Platinum black does not absorb it, nor does platinum sponge, and wet oxidizing and chlorinating agents, such as nitro-hydrochloric acid, bromine water, bromine and alkali, and hydrochloric acid and potassium permanganate, are entirely without action. Experiments with fluorine are in contemplation, but the difficulty is great; and an attempt will be made to produce a carbon arc in the gas. Mixtures of sodium and silica and of sodium and boracic anhydride are also without action, hence it appears to resist attack by nascent silicon and by nascent boron.

XVI. *General Conclusions.*

It remains, finally, to discuss the probable nature of the gas, or mixture of gases, which we have succeeded in separating from atmospheric air, and which we provisionally name *argon*.

The presence of argon in the atmosphere is proved by many lines of evidence. The high density of "atmospheric nitrogen," the lower density of nitrogen from chemical sources,

* Kundt and Warburg, Pogg. Ann. vol. CXXXV, pp. 337 and 527.

and the uniformity in the density of samples of chemical nitrogen prepared from different compounds, lead to the conclusion that the cause of the anomaly is the presence of a heavy gas in air. If that gas possess the density 20 compared with hydrogen, "atmospheric" nitrogen should contain of it approximately 1 per cent. This is, in fact, found to be the case. Moreover, as nitrogen is removed from air by means of red-hot magnesium, the density of the remaining gas rises proportionately to the concentration of the heavier constituent.

Second. This gas has been concentrated in the atmosphere by diffusion. It is true that it has not been freed from oxygen and nitrogen by diffusion, but the process of diffusion increases, relatively to nitrogen, the amount of argon in that portion which does not pass through the porous walls. This has been proved by its increase in density.

Third. As the solubility of argon in water is relatively high, it is to be expected that the density of the mixture of argon and nitrogen, pumped out of water along with oxygen, should, after the removal of the oxygen, be higher than that of "atmospheric" nitrogen. Experiment has shown that the density is considerably increased.

Fourth. It is in the highest degree improbable that two processes, so different from each other, should manufacture the same product. The explanation is simple if it be granted that these processes merely eliminate nitrogen from an "atmospheric" mixture. Moreover, as argon is an element, or a mixture of elements, its manufacture would mean its separation from one of the substances employed. The gas which can be removed from red-hot magnesium in a vacuum has been found to be wholly hydrogen. Nitrogen from chemical sources has been practically all absorbed by magnesium, and also when sparked in presence of oxygen; hence argon cannot have resulted from the decomposition of nitrogen. That it is not produced from oxygen is sufficiently borne out by its preparation by means of magnesium.

Other arguments could be adduced, but the above are sufficient to justify the conclusion that argon is present in the atmosphere.

The identity of the leading lines in the spectrum, the similar solubility and the similar density, appear to prove the identity of the argon prepared by both processes.

Argon is an element, or a mixture of elements, for Clausius has shown that if K be the energy of translatory motion of the molecules of a gas, and H their whole kinetic energy, then

$$\frac{K}{H} = \frac{3(C_p - C_v)}{2C_v},$$

C_p and C_v denoting as usual the specific heat at constant pressure and at constant volume respectively.

Hence if, as for mercury vapor and for argon (§XIV), the ratio of specific heats $C_p : C_v$ be $1\frac{2}{3}$, it follows that $K = H$, or that the whole kinetic energy of the gas is accounted for by the translatory motion of its molecules. In the case of mercury the absence of interatomic energy is regarded as proof of the monatomic character of the vapor, and the conclusion holds equally good for argon.

The only alternative is to suppose that if argon molecules are di- or polyatomic, the atoms acquire no relative motion, even of rotation, a conclusion exceedingly improbable in itself and one postulating the sphericity of such complex groups of atoms.

Now a monatomic gas can be only an element, or a mixture of elements; and hence it follows that argon is not of a compound nature.

From Avogadro's law, the density of a gas is half its molecular weight; and as the density of argon is approximately 20, hence its molecular weight must be 40. But its molecule is identical with its atom; hence its atomic weight, or, if it be a mixture, the mean of the atomic weights of that mixture, taken for the proportion in which they are present, must be 40.

There is evidence both for and against the hypothesis that argon is a mixture: for, owing to Mr. Crookes's observations of the dual character of its spectrum; against, because of Professor Olszewski's statement that it has a definite melting point, a definite boiling point, and a definite critical temperature and pressure; and because on compressing the gas in presence of its liquid, pressure remains sensibly constant until all gas has condensed to liquid. The latter experiments are the well-known criteria of a pure substance; the former is not known with certainty to be characteristic of a mixture. The conclusions which follow are, however, so startling, that in our future experimental work we shall endeavor to decide the question by other means.

For the present, however, the balance of evidence seems to point to simplicity. We have therefore to discuss the relations to other elements of an element of atomic weight 40. We inclined for long to the view that argon was possibly one or more than one of the elements which might be expected to follow fluorine in the periodic classification of the elements—elements which should have an atomic weight between 19, that of fluorine, and 23, that of sodium. But this view is completely put out of court by the discovery of the monatomic nature of its molecules.

The series of elements possessing atomic weights near 40 are :—

Chlorine	35·5
Potassium	39·1
Calcium	40·0
Scandium	44·0

There can be no doubt that potassium, calcium, and scandium follow legitimately their predecessors in the vertical columns, lithium, beryllium, and boron, and that they are in almost certain relation with rubidium, strontium, and (but not so certainly) yttrium. If argon be a single element, then there is reason to doubt whether the periodic classification of the elements is complete; whether, in fact, elements may not exist which cannot be fitted among those of which it is composed. On the other hand, if argon be a mixture of two elements, they might find place in the eighth group, one after chlorine and one after bromine. Assuming 37 (the approximate mean between the atomic weights of chlorine and potassium) to be the atomic weight of the lighter element, and 40 the mean atomic weight found, and supposing that the second element has an atomic weight between those of bromine, 80, and rubidium, 85·5, viz: 82, the mixture should consist of 93·3 per cent. of the lighter, and 6·7 per cent. of the heavier element. But it appears improbable that such a high percentage as 6·7 of a heavier element should have escaped detection during liquefaction.

If it be supposed that argon belongs to the eighth group, then its properties would fit fairly well with what might be anticipated. For the series, which contains

$$Si_n^{IV},\quad P_4^{III\text{ and }V},\quad S_{8\text{ to }2}^{II\text{ to }VI},\quad \text{and}\quad Cl_2^{I\text{ to }VII},$$

might be expected to end with an element of monatomic molecules, of no valency, *i. e.* incapable of forming a compound, or if forming one, being an octad; and it would form a possible transition to potassium, with its monovalence, on the other hand. Such conceptions are, however, of a speculative nature; yet they may be perhaps excused, if they in any way lead to experiments which tend to throw more light on the anomalies of this curious element.

In conclusion, it need excite no astonishment that argon is so indifferent to reagents. For mercury, although a monatomic element, forms compounds which are by no means stable at a high temperature in the gaseous state; and attempts to produce compounds of argon may be likened to attempts to cause combination between mercury gas at 800° and other elements. As for the physical condition of argon, that of a gas,

we possess no knowledge why carbon, with its low atomic weight, should be a solid, while nitrogen is a gas, except in so far as we ascribe molecular complexity to the former and comparative molecular simplicity to the latter. Argon, with its comparatively low density and its molecular simplicity, might well be expected to rank among the gases. And its inertness, which has suggested its name, sufficiently explains why it has not previously been discovered as a constituent of compound bodies.

We would suggest for this element, assuming provisionally that it is not a mixture, the symbol A.

We have to record our thanks to Messrs. Gordon, Kellas, and Matthews, who have materially assisted us in the prosecution of this research.

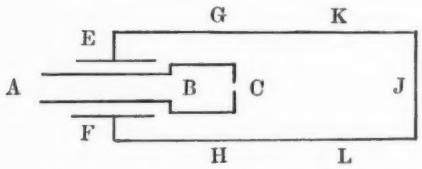
Art. XXV.—*The Velocity of Electric Waves;* John Trowbridge and William Duane.

Sometime since the following method of measuring the velocity of electric waves suggested itself to us; increase the size, and if necessary change the shape of an ordinary Hertz vibrator until the period of oscillation is sufficiently long to be determined by photographing the spark; measure the length of the waves induced in a secondary circuit tuned to resonance with the vibrator; and the quotient of the wave length by the time of a complete oscillation will be the required velocity.

The first apparatus experimented with was that used by Mr. St. John in investigating the peculiarities of waves along iron wires. For a detailed description of this apparatus see this Journal for October, 1894. It has been assumed by certain writers that the reaction between the circuits, arranged in the manner employed by Mr. St. John, is to a large extent, what would be called in the older theory of electricity, electro-magnetic rather than electrostatic. Certain phenomena, however, that appeared in the early part of our investigation seemed to point to the view that the greater portion of the action was electrostatic; and we therefore concluded to arrange the apparatus so that the reaction should be wholly electrostatic, thinking by this means to obtain a more powerful oscillation and a more regular wave than with the apparatus described and used so successfully by Blondlot. The first attempt arranged on the electrostatic principle proved a complete failure. A second trial about a fortnight later, however, proved so successful that we fully determined to adopt the electrostatic method.

The first point in the course of the investigation worthy of detailed notice is the production of electric waves along parallel wires in such a manner that they are actually visible to the eye. The arrangement of the apparatus to accomplish this was as follows:

1.



A primary condenser A B (Fig. 1) was held with its plates in vertical planes by means of suitable wooden supports (not represented in the figure), and was joined in a circuit B C consisting of two wires about 75^{cm} long placed 4^{cm} apart. In reality this circuit B C should be represented as perpendicular to the plane of the paper, (which is taken as the horizontal plane passing through the center of the apparatus). The plates of the condenser A B were of sheets of tin foil 101×40^{cm} glued to hard rubber sheets and the dielectric between them consisted of other similar sheets of hard rubber sufficient in number and thickness to make the distance between the condenser plates 4·2^{cm}. Outside the primary condenser plates and separated from them by hard rubber plates, (total thickness ·6^{cm}) were two secondary plates E and F each 40^{cm} square. To these plates was attached the secondary circuit E G J H F, the form of which is represented in fig. 1. This latter circuit consisted of copper wire, diameter ·13^{cm} and its total length from E to F was 4200^{cm}. A spark gap with spherical terminals 2·5^{cm} in diameter was placed at C in the primary circuit, and another spark gap with pointed terminals was sometimes inserted at J in the secondary circuit, although this latter spark gap had no effect upon the phenomena to be described. The primary condenser was charged by means of a large Ruhmkorf coil excited by five storage cells with a total voltage of ten volts. The current from these cells was made and broken by the automatic interrupter used by Mr. St. John and described by him in his paper above referred to. Every time the primary condenser was charged, a spark passed at C causing an oscillatory discharge. These oscillations induced charges on the plates E and F which were rapidly reversed in sign, and which traveled out along the wires E G and F H. The dimensions of the primary circuit were altered, until by trial it was found that a charge of, let us say, positive electricity starting from E

would meet at the point G one of negative electricity that had previously started from F and traveled around F H J G, another at the point J, and still another at the point H. Stationary vibrations in the circuit E G J H F were thus set up with nodes at the points G, J and H, and ventral segments halfway between them at K and L. The method of discovering when the circuits were in tune, and of investigating the shape of the waves will be described later. The point to be noticed here is that the vibrations were sufficiently powerful to cause a luminous discharge on the surface of the wire at points where the accumulation of electricity was a maximum, i. e. at K and L, while at the nodal points G, J and H the wire was entirely dark. Still further the wave formation could be made apparent to the sense of hearing as well as that of sight; for placing the ear within a few centimeters of the wire and walking along it, a distinct crackling sound could be heard at the points K and L whereas no such sound could be heard at G, J and H. By placing bits of glass tubing on the wire the sound was much intensified at the points K and L, and the phenomena made more striking. It might be supposed that by decreasing the capacity of the primary condenser, and therefore the period of its oscillation, the secondary circuit could be broken up into a new set of shorter stationary waves, with nodes at J and at points somewhere near K, L, G and H, and ventral segments between them. This we tried with perfect success except that it was not possible to cause the light at K and L to actually disappear. There was decidedly less light at these points however than on either side of them. The light of course is simply that which always appears around wires carrying very high potential currents, the interesting point being that it appears in some places on the circuit and not in others. The experiment showing how the circuit breaks up in several different ways would form a most beautiful lecture experiment.

As a means of ascertaining when the circuits were in resonance, and of investigating the form of the wave in the secondary circuit a bolometer similar to that designed by Paalzow and Rubens* was used. The bolometer was used in this present investigation to detect electrical disturbances in a conductor and to measure their magnitudes; but it did not indicate either the direction or sign of the quantities measured. Its use for this purpose is described in the paper of Mr. St. John already referred to.

The conductors that were electrically connected to the arm of the bridge, and that were brought near the circuit consisted

* Anwendung des bolometrischen Princips auf Electrische Messungen, Wied. Ann., xxxvii, 529.

of two pieces of wire insulated with rubber, bent into circles of about 2^{cm} radius, and fastened to a bit of pine wood by means of a heavy coating of paraffine. The two wires of the secondary circuit passed through holes in this bit of wood in such a manner as to pass through the centers of the two circles. In the early part of the investigation the bolometer and galvanoscope were placed at a sufficient distance from the oscillating circuits to prevent any direct action of one on the other, and the leads running from the circular conductors to the bolometer consisted of long fine wires. Later when longer circuits and longer waves were experimented with, great inconvenience was experienced from the long leads since their relative position had considerable effect upon the galvanoscope deflections. In order to obviate this difficulty short leads of heavily insulated wire were used and the bolometer was placed on wheels and moved along from place to place. A bolometric study of the circuit just described showed the character of the oscillation to be that mentioned, namely, nodes at the points G, J and H, and maximum accumulations at the points K and L. A careful run was made from one end of the circuit to the other, which furnished data from which a very regular curve was drawn.

Two points deserve notice here, before we pass on to the next arrangement of the apparatus. First the automatic current interrupter that worked so beautifully in connection with the Hertz vibrator would not function well, when the vibrator was replaced by the circuit and condenser just described. For a detailed description of this interrupter we must again refer to Mr. St. John's paper. The essential feature of the apparatus was this: the circuit was interrupted by the regular periodic lifting of a platinum plunger from a glass cup partially filled with mercury. Alcohol was poured over the surface of the mercury in order to keep it clean, and this effectually stopped the sparking when the Hertz vibrator was used. When, however, the induction coil was used to charge the large condensers, violent sparks occurred at the point where the plunger left the mercury, almost any one of which was sufficiently strong to blow the alcohol out of the cup. As the same current in the primary of the induction coil was used in the two cases, the most probable explanation of this is the following. The capacity of the condenser being considerably greater than that of the two plates in the Hertz vibrator, the current in the secondary of the induction coil in the first case must have been much greater than that in the second case. Hence the reaction of the secondary current on the primary of the induction coil, must have been much greater. This would cause a greater reaction of the sec-

ondary current on the primary of the induction coil, and would throw a greater stress on the point of rupture of the primary circuit; hence the sparking. To obviate the difficulty several methods of exciting the induction coil were tried, with more or less success. Finally, an ordinary reed interrupter with a comparatively large hammer and anvil arrangement was adopted, which gave little trouble.

The second point, and this is very important in the light of what is to follow, is the following: the insertion of a small spark gap (1^{mm}–3^{mm}) at the point in the secondary circuit marked—J (fig. 1) had no appreciable effect upon the position of the nodal points G and H, or of the points of maximum accumulation K and L. The form of the wave was slightly altered for a meter on each side of J, and the bolometer showed a slight accumulation in the immediate neighborhood of the spark gap. This was probably due to the charging of the spark terminals to a sufficiently high potential to break through the dielectric. The fact that the insertion of a spark gap, into a secondary circuit in the manner described has no effect upon the length of the waves set up in that circuit was tested for a number of different cases, (in none of which, however, the length of the waves was greater than in the present case) and found to be true in each one of them.

In order to determine the time of vibration we used a concave rotating mirror: and the images of the oscillating sparks were thrown on a sensitive plate. If the mirror rotated about a horizontal axis, the photographs showed bright horizontal lines perpendicular to which at their extremities extended two series of dots. The distance between the successive dots was evidently the distance on the plate through which the image of the spark gap moved during the time of a complete oscillation. Hence by determining the speed of the mirror and measuring the distances from the mirror to the plate the time of oscillation can be calculated.

The advantages of photographing the secondary spark rather than the primary are numerous. In the first place to properly photograph a spark it is necessary to use pointed terminals: but experiment has shown that the waves excited in a secondary circuit depend to a large extent upon the character of the primary spark, and that the most active sparks are those between metallic spheres with polished surfaces. It is true that waves can be produced by sparks between points, but the oscillations are not so powerful or well marked. In the second place, from the results obtained by Bjerknes one would expect the oscillations in the secondary circuit to be much less damped than those in the primary. This expectation has been fully realized. Photographs show from ten to

twelve times as many oscillations in the secondary as in the primary. In the third and by no means the least important case, the question how close the resonance is does not affect the accuracy of the results. By photographing the sparks in the secondary the period of oscillation is determined, not of a circuit that is altered until by trial it is found to have as nearly as possible the same period of vibration as the circuit on which the length of the wave is measured, but that of the circuit along which the wave itself is actually traveling.

The great difficulty to be overcome is the production of secondary oscillations that will produce sparks of sufficient brightness to photograph. It is comparatively an easy task to photograph the primary spark, but in order to photograph the secondary the dimensions of the circuit must be chosen with great care.

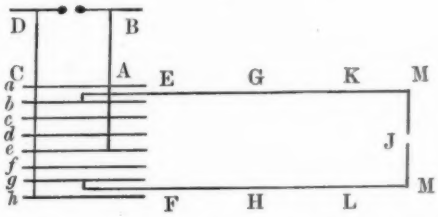
With a view to increasing the light of the spark together with the length of the waves, it seemed desirable to lengthen the period of oscillation by enlarging the condensers rather than by increasing the self induction of the primary circuit. A castor oil condenser was therefore designed and constructed on the following plan. Eight plates ($25^{cm} \times 20^{cm}$) were cut out of sheet zinc, and were held in vertical planes side by side 2^{cm} apart by a suitable hard rubber frame. The plates were entirely immersed in castor oil contained in a glass jar. They were connected together in the manner shown in fig. 2.

The plates marked *a*, *c* and *e* were fastened to the conductor A B and formed one armature of the condenser. Those marked d, f and h were joined to C D and formed the other armature. The two ends of the secondary circuit E, G, J, H, F, were fastened to the plate *b* and *g*. The plane of the secondary circuit was 50^{cm}, and that of the primary 3^{cm} above the upper edge of the condenser plate. The total length of the secondary circuit from one condenser plate through E, G, J, H, F, to the other plate was 6338^{cm}. The circuit consisted of copper wire (diameter $\cdot 215^{cm}$) supported at each end by suitable wooden frames, and also once in the middle by hard rubber hooks fastened by long pieces of twine to a wooden crossbar above. The distances from F to E and from K to L were 30^{cm} and a spark gap with pointed tin terminals was inserted at J. The primary circuit consisted of copper wire (diameter $\cdot 34^{cm}$). The distances between the two parts A B and C D was 45^{cm}. The portion B D contained a spark gap with platinum-faced spherical terminals, and was made so as to slide back and forth, to and from the condenser. The motion of this movable piece varied the self induction and therefore the period of oscillation of the primary circuit. By this means the circuits were brought into resonance. With

certain arrangements of the condensers, the resonance was very sharp, and the position of the movable portion could be determined to within ·25^{cm}. In the arrangement which was finally adopted the resonance was not so sharp. Even in this case the distance of the sliding part from the plate *a* could not have been in error by more than 2^{cm}. The length 65^{cm} was finally chosen for its value.

At first it was found impossible to produce anything but a complex vibration in the secondary circuit when the spark gap was open. Some slight evidence of resonance was obtained, but nothing of a decided character. When, however, the spark gap was closed, very good resonance ensued and a wave, the length of which could be measured to within ·4 per cent, was excited. Some photographs were taken of the spark in the secondary circuit, and they showed immediately the character of the complex wave formation. The secondary circuit could and did oscillate in three different ways, and the ratios of the periods were those of the notes in an open organ pipe, namely 1 : 2 : 3. Usually the lowest or fundamental oscillation together with one of the overtones was present; but several sparks were noticed that furnished unmistakable evidence of the simultaneous existence of all three. We have observed in a circuit 10000^{cm} long the same peculiarities of oscillation, excited by a primary circuit that, judging from its dimensions, could not have been in resonance with the secondary. It was evident that the oscillation having nodes at the points marked (fig. 2) E and F is that whose period is $\frac{1}{3}$ of the fundamental.

2.



A number of measurements of this period have been made, and from these several values the velocity of the waves has been calculated. The results appear in the following table. As an average of five measurements of the wave length, none of which differed from the mean by more than 20^{cm}, the value 5888^{cm} was chosen. The distance from the mirror to the photographic plate in each case except the last was 300·1^{cm}. Each of the first five values in the second column of the table is an average of 30 measurements of distances ranging in the neighborhood of 1^{cm}. The speed of the mirror

was determined to one part in five hundred by means of an electric chronograph.

Number of revolutions of mirror per sec.	Distance between two successive points on plate. cm.	Velocity of Waves. cm.
71·2	·05608	$2·819 \times 10^{10}$
70·85	·05600	$2·810 \times 10^{10}$
70·7	·05532	$2·835 \times 10^{10}$
71·3	·05637	$2·808 \times 10^{10}$
70·8	·05611	$2·808 \times 10^{10}$
	Average.	$2·816 \times 10^{10}$
69·2	·05340	$2·988 \times 10^{10}$

The last line in the table contains the results of measurements on photographs of the primary spark instead of the secondary. In this case the distance from the mirror to the photographic plate was 311·5cm. In spite of the fact that the last value of the velocity is much nearer that of the velocity of light, and of the ratio of the two systems of electrical units than the average of the first five, we do not think it can be relied upon, for two reasons. First because of the possible error introduced by the fact that the two circuits had not exactly the same period of oscillation; and second because the distances measured on the photographic plate were only about ·05cm, instead of 1·00cm.

The generally accepted value for the velocity of light is $2·998 \times 10^{10}$ centimeters. At present it does not seem to us likely, judging from the table as it stands, and from a consideration of the possible errors in the various measurements, that the total error in our determination can be as great as the difference between the average just given and $2·998 \times 10^{10}$.

Whether this discrepancy is due to the fact that the circuit may not have been long enough in comparison with the length of the waves to allow of their full development, or not, we do not undertake to say. If the bends in the circuit at M and M′ have a retarding effect upon the waves, this fact can be very easily discovered and allowed for. As yet we have not had time to investigate the question. We therefore publish the results above tabulated as a preliminary record, hoping to refine upon the measurements in several important particulars, and to extend the investigation to circuits of different sizes and shapes, one of which will probably be a long circuit of some 300 meters running out of doors, and at a considerable distance above the ground.

In the final paper, too, we hope to publish a great many details of the method, together with some interesting phenom-

ena that have appeared in the photographs, of the primary and secondary sparks.

If it appears, as theory seems to indicate, that electric waves travel in air with the velocity of light, it may be that the latter can be determined more accurately by an electrical and photographic method than by the eye methods which have hitherto been used.

Jefferson Physical Laboratory.

ART. XXVI.—*Epochs and Stages of the Glacial Period;* by WARREN UPHAM.

RENEWED studies of the origin and order in age of our Minnesota drift deposits have led me to the results presented in the following table, which I think will contribute toward a reconciliation and harmony of the lately opposing doctrines (1) of unity and (2) of duality or greater complexity of the Ice age. Unity or continuity of our Pleistocene glaciation, with fluctuations of the ice margin, much greater in the interior of the continent than eastward, appears to me the most acceptable view and statement, when the whole period and the whole drift-bearing area are considered. The evidences of a recession of the ice-sheet in Minnesota about two hundred miles backward from the nearest portions of its former boundary, followed by growth again nearly to its previous limits, are to be found in *The Geology of Minnesota*, final report, volumes I (1884) and II (1888), by index references for "Interglacial formations, drainage and water-courses," etc.

The two stages of growth of the ice-sheet may have been due, aside from their principal dependence on the high elevation of the land, to the last two passages in the precession of the equinoxes, with accompanying nutation, bringing the winters of the northern hemisphere in aphelion about 30,000 years ago and again about 10,000 years ago. The intermediate time of the earth's northern winters in perihelion would be the stage of great retreat of the ice margin in the upper Mississippi region; but eastward, from Ohio to the Atlantic coast, there appears to have been little glacial oscillation.* This explanation accords with Prof. N. H. Winchell's computations from the rate of recession of the falls of St. Anthony for the Post-glacial or Recent period,† and with his estimate of the duration

* J. D. Dana, this Journal, III, vol. xlvi, pp. 327–330, Nov., 1893.

† Geol. and Nat. Hist. Survey of Minnesota, Fifth Ann. Rep. for 1876, pp. 175–189; Final Report, vol. ii. 1888, pp. 313–341, with fifteen plates (views showing recent changes of the falls of St. Anthony, and maps). Quart. Jour. Geol. Soc., London, vol. xxxiv, 1878, pp. 886–901.

of the interglacial stage from the now buried channel which appears to have been then eroded by the Mississippi river a few miles west of the present gorge below these falls.*

Adopting the helpful new nomenclature proposed by Chamberlin,† we may provisionally formulate the minor time divisions of the Glacial and Champlain epochs as follows. The order is stratigraphic, so that for the advancing sequence in time it should be read upward.

Champlain Epoch.—(Land depression; disappearance of the ice-sheet; partial reëlevation of the land.)

WISCONSIN STAGE.—(Progressing reëlevation.) Moderate reëlevation of the land, advancing as a permanent wave from south to north and northeast; continued retreat of the ice along most of its extent, but its maximum advance in southern New England, with fluctuations and the formation of prominent moraines; great glacial lakes on the northern borders of the United States; slight glacial oscillations, with temperate climate nearly as now, at Toronto and Scarboro', Ont.; the sea finally admitted to the St. Lawrence, Champlain, and Ottawa valleys; uplift to the present height completed soon after the departure of the ice. (The great Baltic glacier, and European marginal moraines.)

CHAMPLAIN SUBSIDENCE.—Depression of the ice-covered area from its high Glacial elevation; retreat of the ice from its former Iowan limits; abundant deposition of loess.

Glacial Epoch.—(Ice accumulation, due to the culmination of the Lafayette epeirogenic uplift.)

IOWAN STAGE.—Renewed ice accumulation covering the forest beds and extending south nearly to its early boundary. (Third European glacial stage.)

INTERGLACIAL STAGE.—Extensive glacial recession in the upper part of the Mississippi basin; cool temperate climate and coniferous forests up to the waning ice-border; much erosion of the early drift.

KANSAN STAGE.—Maximum extent of the ice-sheet in the interior of North America, and also eastward in northern New Jersey. (Maximum glaciation in Europe.)

UNDETERMINED STAGES of fluctuation in the general growth of the ice-sheet.—Including an early glacial recession and reädvance shown by layers of interglacial lignite on branches of the Moose and Albany rivers, southwest of James bay. (First glacial stage in the Alps.)

* Am. Geologist, vol. x, pp. 69–80, with three plates (sections and map), Aug., 1892.

† In two chapters (pp. 724–775, with maps forming plates xiv and xv) of J. Geikie's "The Great Ice Age," third edition, 1894, Prof. T. C. Chamberlin proposes a chronologic classification of the North American drift under three formations, named in the order of their age, beginning with the earliest, the Kansan, East Iowan, and East Wisconsin formations.

Art. XXVII.—*Structure and Appendages of Trinucleus;* by Charles E. Beecher. (With Plate III.)

Trinucleus departs so widely from the common type of trilobite form, that any contribution of new facts regarding its structure and appendages is a matter of interest. Moreover, this added information will be of assistance in interpreting some peculiar and striking features in the natural group of genera of which *Trinucleus* is evidently a member.

For the present, it is convenient to consider in this group such forms as *Trinucleus*, *Harpes*, *Harpides*, *Dionide*, and *Ampyx*. Most of these have the genal angles extending to or beyond the pygidium, with a broad, finely perforated or punctate margin around the head. They are further characterized by the absence or obsolescence of visual organs, while the facial sutures are either peripheral, as in *Harpes*, or in addition include the genal spines, as in *Trinucleus*, *Dionide*, and *Ampyx*. Several other genera have been recognized as having affinities with those mentioned, but they are imperfectly known, and will be merely noticed here. *Harpina*, Novák, based upon the features of the hypostoma, is probably of only subgeneric value under *Harpes*. *Arraphus*, Angelin, is apparently based upon a specimen of *Harpes* denuded of the punctate border. *Salteria* of W. Thompson, and *Endymionia* of Billings, both generally considered as closely related to *Dionide*, were founded upon too imperfect material to afford decisive data as to their affinities. Angelin's sub-genera of *Ampyx* (*Lonchodomus*, *Raphiophorus*, and *Ampyx*) are based upon the length of the glabellar spine, and the possession of five or six free thoracic segments. Similar characters in *Trinucleus* are not considered as worthy of such marked distinction.

In 1847, Salter* illustrated and described an eye-tubercle on each cheek of *Trinucleus*, from which there was a raised line extending obliquely upward to a punctum or spot on each side of the glabella. He considered this line as a discontinuous facial suture, but the true suture was afterwards correctly determined by Barrande,† and in well-preserved specimens, may easily be observed, extending around the entire frontal and lateral border of the head, and including the genal spines. The "eye-line" was further recognized by McCoy,‡

* On the structure of Trinucleus, with Remarks on the Species, Quar. Jour. Geol. Soc., vol. iii. pp. 251–254.

† Syst. Sil. Bohême, I.. 1852.

‡ Ann. Mag. Nat. Hist., 2d Series, vol. iv., 1849.

and made one of the bases for a division of the genus into two sections or genera—*Trinucleus* proper and *Tetraspis*. These divisions were accepted by Salter, but later were thoroughly discussed, and rejected by Barrande (*l. c.*, p. 617), upon valid grounds. Nicholson and Etheridge,* in 1879, reviewed these facts at some length, and gave original figures illustrating the ocular tubercle and eye-line. They also agree with Barrande in recognizing them as clearly adolescent characters.

The justice of these conclusions is substantiated, and additional results are reached, from the study of a series of *Trinucleus concentricus* Eaton, found associated with *Triarthrus Becki* Green, in the Utica slate, near Rome, New York. The remarkable preservation of the fossils at this locality, has already afforded a means of determining all the principal details of the ventral structure of the trilobite genus *Triarthrus*, and there is now distinct evidence as to the nature of the appendages in another type—*Trinucleus*, as well as to the probable significance of the so-called "eye-tubercle."

As compared with *Triarthrus*, specimens of *Trinucleus* are not very common at this locality, and, although more than fifty individuals of the latter have been obtained from the collections presented to the Yale Museum by Professor Marsh, not more than half a dozen of these are adult specimens, and but three show any appendages. Young specimens of all ages occur, from about 1^{mm} across the cephalon upwards, and in all the eye-line and eye tubercle are present until a width of nearly 5^{mm} is attained, when in the present species these features dwindle and disappear, leaving no discoverable traces in the adult.

Two cephala of young individuals, without the free cheeks, are shown enlarged in figures 1 and 2 of Plate III. Figure 2 represents a specimen before the appearance of the perforate border, and figure 1 gives a later stage, having two rows of perforations around the head. On both specimens the eye-line is clearly shown, extending somewhat obliquely backward from the anterior lobe of the glabella to the central area of the fixed cheeks, enlarging slightly, and terminating in a rounded node or tubercle (*a*, *a*, figure 2).

In seeking for homologous features in other trilobites, the genera *Harpes* and *Harpides* are immediately suggested, since they have similar ocular ridges extending from the sides of the glabella, and ending in a tubercle, which, in *Harpes*, contains from one to three eye-spots, as determined by Barrande. They further agree in having these visual organs on the

* Monograph of the Silurian Fossils of the Girvan District in Ayrshire, Fasc. II., 1879.

fixed cheeks, while in all other trilobites with distinct eyes, the free cheeks carry the visual areas. This type of eye is thus quite different in its relations to the parts of the cephalon from that of *Phacops* or *Asaphus*, and more nearly resembles the eyes of some of the *Merostomata* (*Bellinurus*), as do also the triangular areas in the young *Trinucleus*, so distinctly marked off from the fixed cheeks on each side of the glabella behind the eye-line. Adult *Trinucleus* and *Harpes* have these areas much reduced, and often obsolescent. A spot or node in the median line on the glabella has been noticed by many observers, and although its nature has not been demonstrated, it has generally been called an ocellus. It is more clearly preserved in adult specimens, though it can be detected in young examples, as indicated in figures 1, 2, Plate III.

An eye-line occurs in many early trilobite genera, and is well marked in *Conocoryphe*, *Olenus*, *Ptychoparia*, and *Arethusina*. At least four-fifths of the Cambrian forms preserve this feature, which is almost entirely eliminated before Devonian time. It differs in extent, but not necessarily in nature, from the eye-line of *Trinucleus* and *Harpes* in running entirely across the fixed cheeks to the free cheeks, ending in the palpebral lobe in eyed forms. It is evidently a larval character in the trilobites, as shown from its geological history and the ontogeny of *Trinucleus*. From the direction of the optic nerve in *Limulus*, and its relations to the surface features of the cephalothorax, the eye-line probably represents the course of that nerve, and is of much less morphological importance than the different types and arrangement of visual organs.

The pygidium of young *T. concentricus* (Plate III, figure 3) is remarkable for the lack of definition between the axis and pleura. In later and adult stages the number of ridges on the pleura and axis do not correspond, and from figures 4, 5, and 6, it is evident that in this genus the number of pleura is no indication of the number of pygidial segments or pairs of appendages, which, however, may be shown, as in this case, by the annulations of the axis. In this respect, the pygidia in *Encrinurus*, *Cybele*, and *Dindymene*, are of the same nature. Figure 6 also shows a narrow, striated doublure, a character generally overlooked in descriptions of *Trinucleus*.

Appendages.

Three specimens have thus far been observed which show the nature of the appendages in *Trinucleus*. Two of these are illustrated in figures 4, 5, and 6, of Plate III. Figure 4 represents the thorax and pygidium viewed from the dorsal

side. In this specimen the pyrite which replaced the chitinous remains of the animal has decomposed, and the dorsal crust weathered away, exposing below the stems of the exopodites, with their fringes extending over the entire pleural areas on both sides. A pygidium, with three attached thoracic segments, from another entire specimen (figures 5 and 6), preserves the details of the appendages in the most perfect and satisfactory manner. As both halves showed essentially the same extent and disposition of the fringes on the dorsal side, the specimen was cut in two along the center of the axis, and the left side was then imbedded in paraffine. By careful preparation the appendages were exposed from the ventral side.

The cephala of the three specimens described are considerably compressed, and from them a very imperfect knowledge of the mouth parts could be obtained, so that this information must be left to future discovery.

Endopodites.—The three posterior thoracic endopodites are very similar, and in a general way closely resemble those of *Triarthrus* from the same region of the thorax. They are, however, comparatively shorter and stouter, and could not be extended beyond the ends of the pleura. The two distal joints are cylindrical, with well-marked articular surfaces and ridges. The joints preceding these proximally become much wider, flattened, and produced into transverse extensions which carry large tufts of setæ at the end, as also does the end of the last joint of the limb (dactylopodite).

The endopodites on the pygidium offer no conspicuous differences from those just described, except that a gradual change in form is manifest as the terminal limbs are reached. The separate endites become more and more transversely cylindrical, until the whole limb appears to be made up of cylindrical segments transverse to its length. A similar condition was observed in the young of *Triarthrus.**

Exopodites.—These seem to be composed of slender joints, the distal exites being long and slightly curved outwards. They carry very long, close set, overlapping, lamellose fringes, which evidently had a *branchial* function. Some of the lamellæ are spiniferous. The exopodites become shorter on the pygidium, and apparently are represented near the end of the series of limbs by the oval plates indicated at *c*, figure 6. If this interpretation is correct, the posterior exopodites are simple flabella attached to the limbs, as in *Apus*.

Both Professors A. E. Verrill and S. I. Smith agree that the characters of the appendages in *Trinucleus* indicate an animal of burrowing habit, which probably lived in the soft

* This Journal, vol. xlvii, Pl. VII, fig. 3, April, 1894.

mud of the sea bottom, much after the fashion of the modern *Limulus*. In addition to its limuloid form, the absence of eyes seems to favor this assumption. So does the fact that many specimens have been found preserving the cast of the alimentary canal, showing that the animal gorged itself with mud like many other sea-bottom animals.

Yale Museum, New Haven, Conn., March 15th, 1895.

EXPLANATION OF PLATE III.

Trinucleus concentricus Eaton.

FIGURE 1.—Cephalon of young individual without genal spines; showing ocular ridges and two rows of perforations around anterior and lateral borders. × 40.

FIGURE 2.—Cephalon of younger individual before the growth of the perforate border; showing distinctly the clavate ocular ridges, *a*, *a*. × 40.

FIGURE 3.—Pygidium of young individual; showing the indistinct limitation of axis and the elevated transverse ridges of the pleura and axis. × 40.

FIGURE 4.—Thorax and pygidium of an entire specimen from which the dorsal test has been removed by weathering, exposing below the fringes of the exopodites, which entirely cover the pleural portions. The stronger lines ascending from the axis are the main stems of the exopodites. The black dots along the axis are the fulcra for the attachment of the limbs. × 4.

FIGURE 5.—One-half the pygidium with three attached thoracic segments, from an entire specimen, with a portion of the test removed; showing the highly developed, lamellose fringes of the exopodites. × 11.

FIGURE 6.—The same; lower side; showing the short, stout, phyllopodiform endopodites, *a*, and the long, slender, exopodites, *b*, bearing the lamellose branchial fringes. In the lower third of the figure the ends of the joints of the separate endopodites are shown by the oblique ascending rows of setiferous nodes. The small ovate organs (*c*) along the side are provisionally correlated with the exopodites. A narrow striated doublure margins the pygidium and the ends of the thoracic pleura. × 11.

Utica slate. Near Rome, N. Y.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On the Inorganic Preparation of Hydrazine.*—Hitherto the preparation of hydrazine has been possible only from complex organic compounds. DUDEN however has now succeeded in effecting its synthesis from inorganic materials. For this purpose he makes use of a compound originally discovered by Davy, produced by the action of sulphurous acid upon potassium nitrite, and which has the composition $K_2SO_3.N_2O_2$. And he finds that this substance, upon careful reduction with sodium amalgam or with zinc dust and ammonia or soda, at a low temperature, gives a solution having very strong reducing properties and which yields

after acidification, the salt of hydrazine corresponding to the acid employed. In practice the recently prepared compound of nitrogen dioxide and potassium sulphite is suspended in water cooled by ice, the whole is placed in a freezing mixture and sodium amalgam is gradually added until the liquid is found to reduce Fehling's solution strongly and to yield, after being acidified and heated to expel the sulphur dioxide a precipitate of benzalazine on the addition of benzaldehyde. The benzalazine thus obtained is identical with that described by Curtius, fusing at 93° and having the formula $(C_6H_5CHN)_2$. This substance treated with sulphuric acid yields hydrazine sulphate $(N_2H_4)_2 . H_2SO_4$, of melting point 256°, and otherwise identical with the product obtained from organic sources. The reaction appears to take place in two stages. In the first

$$\begin{matrix}KSO_3\\KO\end{matrix}\!\!>\!N . NO + H_6 = \begin{matrix}KSO_3\\H\end{matrix}\!\!>\!N . NH_2 + H_2O + KOH$$

Then a subsequent reaction takes place between the alkali and the sulphite compound thus

$$\begin{matrix}KSO_3\\H\end{matrix}\!\!>\!N . NH_2 + KOH = K_2SO_4 + H_2N . NH_2$$

—*Ber. Berl. Chem. Ges.*, xxvii, 3498, January, 1895. G. F. B.

2. *On the Production of Carbon chlorides at ordinary Temperatures.*—The production of C_2Cl_4 and C_2Cl_6 by the dissociation of carbon tetrachloride at a red heat, with the setting free of chlorine is well known. Victor Meyer has now called attention to the fact that during the preparation of carbon tetrachloride by the chlorination of carbon disulphide at ordinary temperatures, these two chlorides are produced. At these works of Müller and Dubois, near Mannheim, this process is operated on the large scale, at temperatures between 20° and 40°. After some days, the liquid becomes deeply colored owing to the production of sulphur dichloride S_2Cl_2. The tetrachloride is then distilled off leaving the chloride of sulphur. On rectification of the tetrachloride an oily liquid having a higher boiling point, is obtained. Upon fractioning this the author finds that it separates into three constituents, CCl_4, C_2Cl_4 and C_2Cl_6, the last being a solid, and being thus obtained in crystals, practically pure. Since the carbon disulphide also was practically pure, the author considers that the chlorides C_2Cl_4 and C_2Cl_6 are produced by direct synthesis, as follows:

$$(CS_2)_2 + Cl_{10} = C_2Cl_6 + (S_2Cl_2)_2$$
$$(CS_2)_2 + Cl_8 = C_2Cl_4 + (S_2Cl_2)_2$$

—*Ber. Berl. Chem. Ges.*, xxvii, 3160, November, 1894. G. F. B.

3. *On the Atomic masses of Nickel and Cobalt.*—In the earlier determinations of the atomic masses of nickel and cobalt, made by Winkler, he obtained the values 58·90 for the former metal and 59·67 for the latter; the results being secured by analysis of the chlorides prepared from electrolytically deposited metals.

He now finds that a small error was introduced in the case of cobalt, due to the fact that the metal deposited on the platinum electrode contained a minute quantity of the hydrate $Co_2O_3 \cdot (H_2O)_3$. No such result however occurs with nickel. Moreover he finds that a solution of iodine in potassium iodide of decinormal strength is capable of dissolving the deposited metal from the platinum terminal at once, without attacking the latter. In the case of nickel the platinum is left perfectly clean, while after the removal of the cobalt a stain remains due to about one half per cent of oxide. To remove this oxide, the electrodeposited cobalt was reduced by hydrogen before use; and then it proved to be pure on solution in iodine. The determination was made by titrating with sodium thiosulphate the excess of iodine left after the pure metals were dissolved. As a result of two complete and concordant series of analyses the final values obtained are 58·72 for nickel and 59·37 for cobalt, H being 1 and I 126·53; the atomic mass of cobalt being apparently about one-half a unit higher than that of nickel.—*Zeit. anorg. Chem.*, viii, i, December, 1894. G. F. B.

4. *On the Atomic Mass of Bismuth.*—More than forty years ago SCHNEIDER fixed the mass of the bismuth atom as 208, relative to that of hydrogen. A few years subsequently, i. e. in 1859, Dumas made atomic mass determinations of a number of elements, among which was bismuth; giving to this metal the value 210. This figure continued to be accepted down to 1883 when Marignac undertook his well known investigations upon atomic mass and by a series of determinations which were carried out with great thoroughness concluded upon 208·16 as the atomic mass of bismuth; thus corroborating the work of Schneider. In consequence of the slightly higher result 208·9, obtained by Classen by an electrolytic method, Schneider has now repeated and extended his work in this direction. The method adopted by him in this new series of determinations is based upon a comparison of the equivalent relation of metallic bismuth to the trioxide of bismuth; with a view of testing certain suggestions made by Classen concerning possible errors in his former estimations. The result finally obtained, for O = 16, is 208·05; the greatest divergence from this mean among the values obtained in all the experiments being only 0·21. This result not only confirms the value originally obtained by Schneider himself, and also that of Marignac, but it is specially important as tending to show that bismuth belongs to the increasing class of elements whose atomic masses are represented by whole numbers.—*J. prakt. Ch.*, II, l, 461, November, 1894. G. F. B.

5. *On the Use of Dihydroxytartaric acid as a Reagent for Sodium.*—By oxidizing tartaric acid in presence of iron, FENTON observed the production of a new crystallized acid, which by oxidation is converted into dihydroxytartaric acid. To effect this oxidation, the crystallized acid is covered with glacial acetic acid and a solution of bromine in this glacial acid is added drop by

drop with constant shaking, until a faint permanent yellow color appears. On neutralizing with sodium carbonate, a heavy white crystalline precipitate is produced, which after washing and drying, finally in vacuo over sulphuric acid, proved to be sodium dihydroxytartrate. From this salt, by covering it with anhydrous ether and passing dry hydrogen chloride into the mixture, dihydroxytartaric was obtained on evaporation. Owing to the ease with which this acid can now be procured, the author suggests the use of it as a reagent for the detection of sodium. For this purpose, a few crystals of the acid are dissolved in a drop of water on a watch-glass, the solution to be examined is added and if necessary the liquid is neutralized with a drop of ammonia. On stirring with a rod, a white crystalline precipitate of the sodium salt appears, generally in lines as in the detection of potassium by tartaric acid. The test is fairly delicate, a one per cent solution of sodium chloride giving the reaction almost immediately. Neither potassium nor ammonium interferes with the reaction.—*J. Chem. Soc.*, lxvii, 48, January, 1895. G. F. B.

6. *On the Commercial Synthesis of Acetylene.*—The production of the carbides of barium, strontium and calcium, by Moissan in his electric furnace,* seems likely to become of considerable commercial utility. In a paper by Lewes, read before the Society of Arts, he has called attention to the production of acetylene by the action of water upon these carbides as the starting point of important practical developments. Although Wöhler had made calcium carbide by fusing an alloy of zinc and calcium with carbon, and had obtained acetylene from it by the action of water; and although in 1892 Macquenne had made barium carbide by heating together barium carbonate, magnesium powder and charcoal, and still later Travers had made calcium carbide by the action of a high temperature upon a mixture of calcium chloride, carbon and sodium, yet no commercial importance was attached to these processes on account of their expense. But when working with the electric furnace, in the attempt to form alloys of calcium, Willson observed that a mixture of lime and pulverized anthracite, exposed to the high temperature of the arc, fused to a semi-metallic mass, which when thrown into water, effervesced strongly and evolved acetylene, the process became of practical value. The calcium carbide thus produced is a dark gray substance, having a density of 2·262. When pure a pound of it yields 5·5 cubic feet of gas, containing 98 per cent of acetylene. This gas is colorless, with a penetrating odor resembling garlic. It is poisonous, and is soluble in a little less than its own volume of water, and in one-sixth of its volume of alcohol. It has a density of 0·91. It burns with a highly luminous and smoky flame, and liquefies at 0° C. under a pressure of 21·5 atmospheres. When sprayed into the air the liquid evaporates rapidly, absorbing so much heat that a portion of it is converted into a snow-white solid. For illuminating purposes it can be burned only in

* See this Journal, III, xlviii, 506, December, 1894.

small flat-flame burners, and then gives a light of 240 candles when consumed at the rate of five feet per hour. It is claimed that calcium carbide can be produced in this way for about $20 per ton. Since a ton will yield about 11,000 cubic feet of gas, the cost at this rate would be about $1·60 per thousand cubic feet, deducting the value of bye-products. In illuminating value, it would be equivalent to ordinary coal gas at about ten or twelve cents per thousand. Moreover, since acetylene is the starting point for a multitude of organic syntheses, this cheap production is of great importance in chemical industry.—*Nature*, li, 303, January, 1895. G. F. B.

7. *Theoretical Chemistry from the Standpoint of Avogadro's rule and Thermodynamics.* By Prof. WALTER NERNST, PH.D., of the University of Göttingen. Translated by Prof. CHARLES SKEELE PALMER, PH.D., of the University of Colorado, 8vo, pp. xxvi, 697. London and New York, 1895 (Macmillan & Co.), $5. —Dr. Nernst is well known as one of the leaders of the new School of Physical Chemists. His papers upon subjects within this domain have received marked attention and have made him an authority in this branch of chemistry. A book from his pen, like the one now before us, therefore, cannot fail to be of great service in advancing chemical science and will, no doubt, be warmly welcomed by his co-laborers in every land. It is divided into four principal divisions or books, preceded by an introduction upon matter and energy and their relations. The first book treats of the universal properties of matter, such as the gaseous, the liquid and the solid states of aggregation, the physical mixture and dilute solutions. The second book considers the atom and the molecule, taking up successively the atomic theory, the kinetic theory of the molecule, the determination of molecular weight, the constitution of the molecule, the relation between physical properties and molecular structure, the dissociation of gases and electrolytic dissociation, the physical properties of salt solutions, and the absolute size of molecules. Book third discusses the transformation of matter, being the first part of the doctrine of affinity; its chapters being upon the laws of chemical mass-action, the chemical statics of homogeneous and heterogeneous systems, chemical equilibrium in salt solutions and chemical kinetics. Book fourth is devoted to the transformation of energy, being the second part of the doctrine of affinity; its first five chapters treating of thermochemistry and its last two of electrochemistry and photochemistry respectively. Two valuable appendices complete the work. One, edited by Dr. Nernst himself, contains important matter which has appeared since the publication of the German edition. The other, edited by Dr. Kaiser, is a synchronistic comparison of the chief periodicals bearing on this department of chemistry. From this résumé will appear at once not only how wide is the range of subjects treated in this volume, but also how clear and logical is the order in which they are taken up. Dr. Nernst everywhere speaks with

the authority of a master of his subject. So that his book, notwithstanding the treatises of Ostwald and others on Physical Chemistry, seems to us, in the excellence of its arrangement, the clearness of its style and the thoroughness of its subject-matter, to be the best book of its kind which has yet appeared. Dr. Palmer deserves especial thanks for putting the book so admirably into its English dress. Typographically also, the book is a credit to its publishers. G. F. B.

8. *Qualitative Chemical Analysis of Inorganic Substances*, as practiced in Georgetown College, D. C. Short 4to, pp. 61. New York, 1894 (American Book Company).—This book consists of a series of tables for qualitative analysis, divided into four sets. The first is on Basic Analysis, the second on Acid Analysis, the third on Preliminary Examination, and the fourth on Solution and on Special treatment. Though in the main following well established authorities, yet there is some originality of arrangement and some satisfactory explanatory matter added to the tables. The book appears to have been prepared with considerable care. G. F. B.

9. *Double refraction of Electric waves.*—K. Mack by interposing pieces of wood between Hertz's well known parabolic reflectors, the axes of which are inclined to each other, has shown that electric waves can be doubly refracted. Most specimens of wood have a different structure along the direction of the fibres from that perpendicular to this direction, and accordingly resemble in this respect doubly refracting crystals possessing a structure parallel to their optic axis different from that at right angles to this axis. When pieces of wood about 20^{cm} thick were interposed on the line joining the foci of the mirrors, clear evidence of the doubly refracting properties of the wood could be shown by the appearance or disappearance of the spark in the micrometer connected with the receiving mirror.—*Ann. der Physik. und Chemie*, No. 2, 1895, pp. 342–351. J. T.

10. *National Academy of Sciences on Electrical Measurement.*—The standard specifications for the practical application of the definitions of the electrical units, ampere and volt, referred to in the act of Congress of July 12, 1894, quoted in the last number of this Journal (p. 236), are given below; they are taken from Miscellaneous Document, No. 115, of the Senate of the United States. These specifications were approved by all the members of the committee named, of which Prof. H. A. Rowland was the chairman, and were unanimously adopted by the Academy at a special meeting held in New York on the 9th of February, 1895.

Specifications for the practical application of the definitions of the Ampere and Volt.

Specification A.—*The Ampere.*

In employing the silver voltameter to measure currents of about one ampere, the following arrangements shall be adopted:

The kathode on which the silver is to be deposited shall take the form of a platinum bowl not less than 10 centimeters in diameter, and from 4 to 5 centimeters in depth.

The anode shall be a disc or plate of pure silver some 30 square centimeters in area and 2 or 3 millimeters in thickness.

This shall be supported horizontally in the liquid near the top of the solution by a silver rod riveted through its center. To prevent the disintegrated silver which is formed on the anode from falling upon the kathode, the anode shall be wrapped around with pure filter paper, secured at the back by suitable folding.

The liquid shall consist of a neutral solution of pure silver nitrate, containing about 15 parts by weight of the nitrate to 85 parts of water.

The resistance of the voltameter changes somewhat as the current passes. To prevent these changes having too great an effect on the current, some resistance besides that of the voltameter should be inserted in the circuit. The total metallic resistance of the circuit should not be less than 10 ohms.

Method of Making a Measurement. The platinum bowl is to be washed consecutively with nitric acid, distilled water and absolute alcohol; it is then to be dried at 160° C., and left to cool in a desiccator. When thoroughly cool it is to be weighed carefully.

It is to be nearly filled with the solution and connected to the rest of the circuit by being placed on a clean insulated copper support to which a binding screw is attached.

The anode is then to be immersed in the solution so as to be well covered by it and supported in that position; the connections to the rest of the circuit are then to be made.

Contact is to be made at the key, noting the time. The current is to be allowed to pass for not less than half an hour, and the time of breaking contact observed.

The solution is now to be removed from the bowl and the deposit washed with distilled water and left to soak for at least six hours. It is then to be rinsed successively with distilled water and absolute alcohol and dried in a hot-air bath at a temperature of about 160° C. After cooling in a desiccator it is to be weighed again. The gain in mass gives the silver deposited.

To find the time average of the current in amperes, this mass, expressed in grams, must be divided by the number of seconds during which the current has passed and by 0·001118.

In determining the constant of an instrument by this method, the current should be kept as nearly uniform as possible and the readings of the instrument observed at frequent intervals of time. These observations give a curve from which the reading corresponding to the mean current (time-average of the current) can be found. The current, as calculated from the voltameter results, corresponds to this reading.

The current used in this experiment must be obtained from a battery and not from a dynamo, especially when the instrument to be calibrated is an electrodynamometer.

SPECIFICATION B.—*The Volt.*

Definition and Properties of the Cell. The cell has for its positive electrode, mercury, and for its negative electrode, amalgamated zinc; the electrolyte consists of a saturated solution of zinc sulphate and mercurous sulphate. The electromotive force is 1·434 volts at 15° C., and between 10° C. and 25° C., by the increase of 1° C. in temperature, the electromotive force decreases by ·00115 of a volt.

1. *Preparation of the Mercury.* To secure purity it should be first treated with acid in the usual manner and subsequently distilled *in vacuo.*

2. *Preparation of the Zinc Amalgam.*—The zinc designated in commerce as "commercially pure" can be used without further preparation. For the preparation of the amalgam one part by weight of zinc is to be added to nine (9) parts by weight of mercury and both are to be heated in a porcelain dish at 100° C. with moderate stirring until the zinc has been fully dissolved in the mercury.

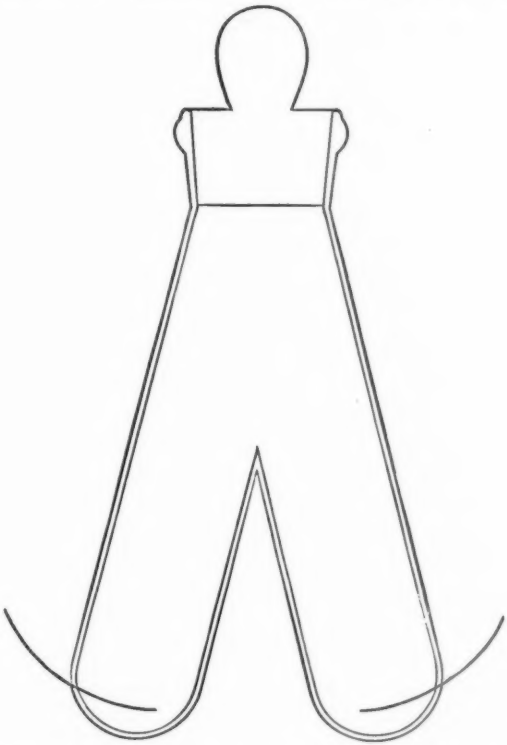
3. *Preparation of the Mercurous Sulphate.* Take mercurous sulphate, purchased as pure, mix with it a small quantity of pure mercury and wash the whole thoroughly with cold distilled water by agitation in a bottle; drain off the water and repeat the process at least twice. After the last washing, drain off as much of the water as possible. (For further details of purification, See Note A.)

4. *Preparation of the Zinc Sulphate Solution.* Prepare a neutral saturated solution of pure re-crystallized zinc sulphate, free from iron, by mixing distilled water with nearly twice its weight of crystals of pure zinc sulphate and adding zinc oxide in the proportion of about 2 per cent. by weight of the zinc sulphate crystals to neutralize any free acid. The crystals should be dissolved with the aid of gentle heat, but the temperature to which the solution is raised must not exceed 30° C. Mercurous sulphate, treated as described in 3, shall be added in the proportion of about 12 per cent by weight of the zinc sulphate crystals to neutralize the free zinc oxide remaining, and then the solution filtered, while still warm, into a stock bottle. Crystals should form as it cools.

5. *Preparation of the Mercurous Sulphate and Zinc Sulphate Paste.* For making the paste, two or three parts by weight of mercurous sulphate are to be added to one by weight of mercury. If the sulphate be dry, it is to be mixed with a paste consisting of zinc sulphate crystals and a concentrated zinc sulphate solution, so that the whole constitutes a stiff mass, which is permeated throughout by zinc sulphate crystals and globules

of mercury. If the sulphate, however, be moist, only zinc sulphate crystals are to be added; care must, however, be taken that these occur in excess and are not dissolved after continued standing. The mercury must, in this case also, permeate the paste in little globules. It is advantageous to crush the zinc sulphate crystals before using, since the paste can then be better manipulated.

To set up the Cell. The containing glass vessel, represented in the accompanying figure, shall consist of two limbs closed at bottom and joined above to a common neck fitted with a ground glass stopper. The diameter of the limbs should be at least 2^{cms} and their length at least 3^{cms}. The neck should be not less than $1{\cdot}5^{cms}$ in diameter. At the bottom of each limb a platinum wire of about $0{\cdot}4^{mm}$ diameter is sealed through the glass.



To set up the cell, place in one limb pure mercury and in the other hot liquid amalgam, containing 90 parts mercury and 10 parts zinc. The platinum wires at the bottom must be com-

pletely covered by the mercury and the amalgam respectively. On the mercury, place a layer one cm. thick of the zinc and mercurous sulphate paste described in 5. Both this paste and the zinc amalgam must then be covered with a layer of the neutral zinc sulphate crystals one cm. thick. The whole vessel must then be filled with the saturated zinc sulphate solution, and the stopper inserted so that it shall just touch it, leaving, however, a small bubble to guard against breakage when the temperature rises.

Before finally inserting the glass stopper, it is to be brushed round its upper edge with a strong alcoholic solution of shellac and pressed firmly in place. (For details of filling the cell, See Note B.)

Notes to the Specifications.

(A.) *The Mercurous Sulphate.* The treatment of the mercurous sulphate has for its object the removal of any mercuric sulphate which is often present as an impurity.

Mercuric sulphate decomposes in the presence of water into an acid and a basic sulphate. The latter is a yellow substance—turpeth mineral—practically insoluble in water; its presence, at any rate in moderate quantities, has no effect on the cell. If, however, it be formed, the acid sulphate is also formed. This is soluble in water and the acid produced affects the electromotive force. The object of the washings is to dissolve and remove this acid sulphate and for this purpose the three washings described in the specification will suffice in nearly all cases. If, however, much of the turpeth mineral be formed, it shows that there is a great deal of the acid sulphate present and it will then be wiser to obtain a fresh sample of mercurous sulphate, rather than to try by repeated washings to get rid of all the acid.

The free mercury helps in the process of removing the acid, for the acid mercuric sulphate attacks it, forming mercurous sulphate.

Pure mercurous sulphate, when quite free from acid, shows on repeated washing a faint yellow tinge, which is due to the formation of a basic mercurous salt distinct from the turpeth mineral, or basic mercuric sulphate. The appearance of this primrose yellow tint may be taken as an indication that all the acid has been removed; the washing may with advantage be continued until this tint appears.

(B.) *Filling the Cell.* After thoroughly cleaning and drying the glass vessel place it in a hot water bath. Then pass through the neck of the vessel a thin glass tube reaching to the bottom to serve for the introduction of the amalgam. This tube should be as large as the glass vessel will admit. It serves to protect the upper part of the cell from being soiled with the amalgam. To fill in the amalgam, a clean dropping tube about 10^{cms} long, drawn out to a fine point, should be used. Its lower end is brought under the surface of the amalgam heated in a porcelain dish, and some of the amalgam is drawn into the tube by means

of the rubber bulb. The point is then quickly cleaned of dross with filter paper and is passed through the wider tube to the bottom, and emptied by pressing the bulb. The point of the tube must be so fine that the amalgam will come out only on squeezing the bulb. This process is repeated until the limb contains the desired quantity of the amalgam. The vessel is then removed from the water bath. After cooling, the amalgam must adhere to the glass and must show a clean surface with a metallic lustre.

For insertion of the mercury, a dropping tube with a long stem will be found convenient. The paste may be poured in through a wide tube reaching nearly down to the mercury and having a funnel-shaped top. If the paste does not move down freely it may be pushed down with a small glass rod. The paste and the amalgam are then both covered with the zinc sulphate crystals before the concentrated zinc sulphate solution is poured in. This should be added through a small funnel, so as to leave the neck of the vessel clean and dry.

For convenience and security in handling, the cell may be mounted in a suitable case so as to be at all times open to inspection.

In using the cell, sudden variations of temperature should, as far as possible, be avoided, since the changes in electromotive force lag behind those of temperature.

II. Geology and Mineralogy.

1. *Change of level in the West Indian Region.*—Mr. C. T. Stimpson has a paper on the Distribution of the land and fresh-water Mollusks of the West Indian Region, and the evidence they afford with regard to past changes of land and sea. He concludes that all the evidence of the terrestrial and fluviatile molluscan fauna of this region indicates that in the early Tertiary period, perhaps, there was a general land elevation of the Greater Antilles, and possibly of some of the adjacent area; that Wallace's theory of a land connection of the greater islands is correct; that during some part of this time a landway extended across to the continent; that the species and groups of this then connected territory migrated to some extent from one part of it to another, and that a probable connection existed over the Bahama plateau to what was at that time no doubt the island of Florida.

Jamaica, by the evidence of its land snails, stands the most isolated of any of the islands; Cuba is the next most so, while those of Haiti and Puerto Rico are much more nearly related to each other than to those of either of the first two. About 20 genera and minor groups are confined to or have their metropolis in Jamaica; a like number belong to Cuba, 7 to Haiti, and 1 to Puerto Rico.

It bears directly on this subject, that the strait between Haiti and Jamaica is deeper than that between any of the other islands,

being nearly 1,000 fathoms in depth; that the strait between Cuba and Haiti, is slightly more shallow, being only about 875 fathoms, while the one between the latter islands and Puerto Rico carries but 260 fathoms. Supposing these islands to have been united at a former time, then, during a period of gradual subsidence, Jamaica would be separated sometime before the rest of the Antillian island would be broken up; then Cuba would be isolated, while Haiti and Puerto Rico would remain united for a longer time. The distribution and character of the land-snail faunas of these islands agree exactly with just what would be the result of such a subsidence and separation.

2. *Glacial phenomena Northwest and West of Hudson Bay.*—Mr. J. B. Tyrrell, of the Canada Geological Survey, concludes, after an examination of the region on the northwest and west of Hudson Bay, and especially from the direction of the glacial scratches, that within a comparatively short distance of the northern portion of the bay, there was "one of the great gathering grounds for the snow of the Glacial period;" and that from the ice-plateau thus made, the movement of the ice was eastward, into the Hudson Bay depression, northward toward the Arctic Ocean, and a long distance westward toward the Mackenzie River. There was also a southward movement "toward the great plains. At this time Hudson Bay was probably to a great extent open water."

After the recession of the ice from the lower country, the land was about 400 feet below its present level. There are terraces at different heights about the lakes. Those of Aberdeen Lake have the heights 290, 220, 180, 150, 105, 90 and 60 feet above it. Similar terraces are found in favorable localities all along the shores of Hudson Bay.

3. *Faults of post-Glacial origin.*—In Bulletin XII of the Natural History Society of New Brunswick (p. 34) Dr. G. F. Matthew describes small faults observed by him over a considerable area in the ledges of slate near St. John. The relations of the faults to the glacial striæ indicates that they are post-Glacial. Their courses vary; but at St. John the greatest throws and the most frequent have a northeast to southwest course, and the more the joints depart from this course the less is the displacement; rarely any occur at right angles to it. The displacements observed are mostly between half an inch and ten inches. Dr. Matthews regards it as probable that the faulting is due to lateral pressure from the southeast.

4. *Pre-Cambrian Radiolarians.*—The paper of L. Cayeux, on Radiolarians in the pre-Cambrian rocks of Brittany (Bull. Soc. Géol. de France, 1894, p. 197) is accompanied by a plate giving figures of 45 of the forms observed. The figures appear to sustain fully the author's conclusion as to the Radiolarian character of the organisms. He describes them as having generally a distinct outer shell, which is pierced by pores. The age of the rocks is pronounced pre-Cambrian by Barrois. They are quartzites,

and compact siliceous slates or phthanite. In a section near Pléboule, the beds are represented by Barrois, as standing nearly vertical and as conformable with beds of argillyte, granulitic gneiss, hornblendic schist, and other rocks. Pebbles of the Radiolarian rock are found in the Cambrian conglomera of Montfort and Erquy and in pre-Cambrian conglomerates at the base of the "Phyllades de Saint Lo;" and from this the conclusion is drawn that the Radiolarian beds are at least pre-Cambrian.

5. *Geological Survey of Alabama:* 1894, Report on the Geology of the Coastal Plain of Alabama by E. A. SMITH, L. C. JOHNSON and D. W. LANGDON, Jr., with contributions to its Paleontology, by T. H. ALDRICH and K. M. CUNNINGHAM, with illustrations, pp. i–xxiv, 1–759, 1894.—The nucleus of the present report was published in 1887 as Bulletin No. 43 of the U. S. Geological Survey, but the present work contains considerable new matter and a revision of the Bulletin in the light of later discoveries.

In the Tertiary part, upon the work of Mr. Johnson, the horizon of the "Grand Gulf" formation has been shown to be of Miocene age, and a new formation at its top, has been described and its age determined to be also Miocene, by Dr. Dall.

The "Tuscaloosa" formation which was described in the Bulletin No. 43, but then only doubtfully referred to some place in the Cretaceous, and since then referred to the lower Cretaceous,* is shown by its fossil plants, discovered in 1892 and identified by Dr. Ward, to be nearly equivalent to the Amboy clays (= Raritan group, Dakota Epoch), the lower member of the Upper Cretaceous. The specimens identified are of species described from the Amboy clays, Dakato group, and Cretaceous of Greenland.

The species of fossils described by Mr. Aldrich are from the (Midway) Clayton Tertiary, of the lowest beds of the Eocene.

H. S. W.

6. *Paleozoic Corallines.*—The first of Paleozoic Algæ of the group of Corallines has been described and figured by R. P. WHITFIELD in the Bulletin of the American Museum of Natural History, vol. vi, p. 351, 1894. He names the single species thus far discovered *Primicorallina trentonensis.*

7. *Lehrbuch der Petrographie* von Dr. FERDINAND ZIRKEL. Zweite gänzlich neu verfasste Auflage. Dritter Band. 833 pp. large 8vo. Leipzig (Wm. Engelmann).—The third volume of this exhaustive work appeared near the close of the past year. The earlier volumes have been already noticed in this Journal and the minute and at the same time comprehensive character of the whole has been dwelt upon. The opening part of this third volume discusses the rocks containing a lime-soda feldspar with nephelite or leucite; those with nephelite, leucite, or melilite without feldspar, and those containing no constituent corresponding to feldspar. The crystalline schists are then taken up, also the crystalline rocks of simple mineralogical character; then

* Dana's Manual of Geology, 4th edition, 1895, p. 816.

follows the discussion of the clastic rocks, that is, the conglomerates, breccias and tuffs of rocks of different types; then the sandstones and sedimentary deposits and finally kaolin, clay, marl, etc. The index for all the three volumes, which closes the work, contains rock-names only and is so brief as to seriously impair the usefulness of the whole. The author is to be heartily congratulated in the completion of his work; the many workers in this department of science will not fail to estimate aright the value of his arduous labors.

8. *Chemical Contributions to the Geology of Canada from the laboratory of the Survey;* by G. CHRISTIAN HOFFMANN (Annual Report, vol. vi, 1892–93, Part R).—Mr. Hoffmann's report contains, besides analyses of fuels, assays of ores and other matters of economic bearing, also a number of points of mineralogical interest. Among these we note the identification of the following minerals, of several of which analyses are given: löllingite from Galway, Peterborough County, Quebec, containing nearly 3 per cent of cobalt and 0·8 per cent of nickel; strontianite from Nepean, Carleton County, Ontario, where it occurs in veins of some extent; also the same mineral from near the Horsefly river, Cariboo district, British Columbia; native iron in minute spherules occurring with the perthite of Cameron, Nipissing, Ontario; pyrargyrite from the Dardanelles claim near Bear Lake, West Kootanie, British Columbia; anglesite from the Wellington mine in the same region; calamine from the Skyline claim, near Ainsworth, West Kootanie; altaite from Liddle Creek, West Kootanie; arsenolite with native arsenic from Watson Creek, British Columbia; cinnabar, perhaps in a large deposit, near the mouth of Copper Creek, Kamloops Lakes, British Columbia.

9. *Meteoritenkunde;* von E. COHEN. Heft 1. Untersuchungsmethoden und Charakteristik der Gemengtheile, 340 pp. 8vo. Stuttgart, 1894 (E. Schweizerbart'sche Verlagshandlung — E. Koch).—This volume forms the first part of a comprehensive work on meteorites, which will be warmly welcomed by all interested in this subject. Such a work is much needed at the present time. In recent years, especially during the past two decades, the literature of the subject has increased remarkably, many investigations after the improved modern methods of research have been made of recent, as of earlier, falls, and the collation and digestion of this vast amount of new material have become a matter of the highest importance. This work obviously involves great labor and calls for the knowledge and experience which are possessed in a high degree by the author.

The present part, which is chiefly devoted to a description of the mineral constituents of meteorites, will be followed by others discussing the structure, external and internal, of meteorites, their classification and finally the phenomena of fall and the hypotheses advanced to explain their nature. The work on this mineralogical side of the subject has been performed with care and thoroughness and the completion of the whole will be looked for with interest.

III. Botany.

1. *Field, Forest and Garden Botany.* By Asa Gray. Revised by L. H. Bailey. Am. Book Co., N. Y. 1895. By the publication of the first edition of a popular treatise on our more common, wild and cultivated plants, Professor Gray met a want which had long been felt. The work was received with pleasure and used with profit by a great number of teachers and pupils throughout the country, and it has ever since held its own. But for some years it has been apparent that the treatise could be made more useful by additions and modifications. It was Professor Gray's intention to undertake this revision himself, but a great increase of care connected with the Synoptical Flora of North America, led him to defer the task, and the wished-for leisure never came. After the death of Professor Gray the revision was taken in hand by one of our energetic systematists and carried by him through a good part of the Polypetalæ. But certain reasons led him to the relinquishment of the work, and so the whole matter remained without change until it was taken up by Professor L. H. Bailey, of Cornell University.

It is apparent that the revision of a treatise constructed on the broad lines of the *Field, Forest and Garden Botany*, presents peculiar difficulties. Not only is it very hard to know what to add and what to leave out, but, at this time, when nomenclature is undergoing so many changes of one kind and another, it is almost impossible to preserve consistency throughout.

Professor Bailey has been successful in a high degree in meeting all these difficulties. Although he is inclined personally to favor one of the new systems of nomenclature, he has preserved in a remarkable manner the system which was preferred by Professor Gray. Moreover, the additions and omissions have been determined with excellent judgment, and have resulted in keeping the treatise on nearly the lines laid down by its author. A careful examination of these changes has convinced the present writer that the proportions have been well maintained throughout. Some species, which it would have been a pleasure to see in the revision, are lacking, and there are some species given which might perhaps have been well spared, but, as a whole, the selection is good, and the book is sure to be of great use to the mass of pupils and amateurs employing it. Professor Bailey is to be sincerely congratulated on his work. G. L. G.

2. *A Popular Treatise on the Physiology of Plants*, for the use of Gardeners or for Students of Horticulture and Agriculture. By Dr. Paul Sorauer, Director of the Experimental Station at the Royal Pomological Institute, in Proskau (Silesia). Translated by F. E. Weiss, B.Sc. F.L.S., Professor of Botany at the Owens College, Manchester. London, Longmans, Green & Co., 1895. Some of our older readers will doubtless remember the valuable Theory of Horticulture, by Professor Lindley, which was introduced to American students in an edition revised and

annotated by Professor Asa Gray. In that work, which was then well up to date, the practice of the gardener was explained as far as might be, and a great amount of thoroughly digested material was placed at the disposal of all interested in cultivating plants. In comparing that work with the present, one is struck by the very slight change in practice which has been demanded by the vast advance in theoretical knowledge. The old rules, many of which were very plainly empirical, still hold, although their *raison d'être*, may be put in a different manner nowadays.

Professor Weiss has given us a clear, idiomatic translation, and with his work no fault can be found. But the original is of very uneven quality. In some places, as for instance, the treatment of manures, the whole might serve as an exercise for correction, but in others, for example, the subject of shoots and their management, all the statements are correct and telling. In the hands of a teacher, this volume can be made of great use in systematizing and coördinating the cardinal facts relative to the vegetative processes, and in applying them to the practical needs of the gardener. G. L. G.

IV. Miscellaneous Scientific Intelligence.

1. *Prize-Question pertaining to Physical Science proposed by the Schnyder von Wartensee Foundation for Arts and Sciences at Zurich.**—The Schnyder von Wartensee Foundation proposes, for the year 1897, the following prize-question concerning problems in the domain of physics.

As the numbers which express the atomic heats of the elements still show very considerable divergences, the researches conducted by Professor H. F. Weber on boron, silicon and carbon, regarding the increase of the specific heat with the temperature, are to be extended to several other elements prepared as pure as possible and also to combinations or alloys of them. Further the densities and the coefficients of thermal dilatation of the substances investigated are to be ascertained as carefully as possible.

The conditions are as follows:

(1.) The treatises handed in by competitors for the prize-question may be either in German, French or English and must be sent in by September 30th, 1897, at the latest to the address given in paragraph 6.

(2.) The examination of the treatises will be entrusted to a jury composed of the following gentlemen: Professors Pernet, Zurich, A. Hantzsch, Wurzburg, E. Dorn, Halle-on-the-Saale, T. Wislicenus, Leipzig; also G. Lunge, Zurich, as member of the committee proposing the prize-question.

(3.) The prize committee has at its disposition a sum of four thousand five hundred francs, of which a first prize, of no less

* For an earlier announcement, for the year 1894, see this Journal, vol. xliii, 240.

than three thousand francs will be awarded and minor prizes for the remaining sum.

(4.) The work to which the first prize is awarded remains the property of the Schnyder von Wartensee Foundation, which has to arrange with the author regarding its publication.

(5.) Every treatise sent in must bear a motto on the title page and be accompanied by a sealed envelope, containing the author's name and bearing the same motto outside.

(6.) The treatises are to be sent into the following address, within the time named in paragraph 1. An das Präsidium des Conventes der Stadtbibliothek in Zurich (concerning prize-question of the Schnyder von Wartensee Foundation, for the year 1897).

Zurich, 31st December, 1894.

By order of the City Library of Zurich. The Committee for the Schnyder von Wartensee Foundation.

2. *American Association for the Advancement of Science.*—A circular from F. W. Putnam, Permanent Secretary, dated Jan. 30, announces that at a special meeting of the Council, held on January 26th, it was decided to postpone the proposed meeting in San Francisco. An invitation from Springfield, Mass., to hold the meeting of 1895 in that city, was accepted. The date of the meeting was fixed as follows: Council meeting, Wednesday, August 28th, at noon; General Sessions, Thursday, August 29th, at 10 A. M. Special efforts will be made by the officers of the sections to prepare program for the sections in advance of the meeting and for this purpose members are requested to send abstracts of their papers, as early as possible, to the Permanent Secretary, or to the Secretaries of the Sections.

3. *International Zoological Congress.*—It is announced that the third meeting of the International Zoological Congress will be held at Leyden in September, 1895. The first meeting took place at Paris in 1889, and the second at Moscow in 1892. The arrangements for the reception and accommodation of the Congress at Leyden will be made by the Netherlands Zoological Society. The answers to invitations to be present and to coöperate are to be sent to Dr. P. P. C. Hoek, Secretary of the Society.

4. *A Manual of the Study of Documents* to establish the individual character of handwriting and to detect fraud and forgery including several new methods of research by PERSIFOR FRAZER. 218 pp. 8vo. Philadelphia, 1894 (J. B. Lippincott Company).—The subject of this volume does not strictly fall within the range of pure science, but Dr. Frazer has treated it with great thoroughness and it is interesting to note some of the methods of examination he has employed, as the application of composite photography to the study of signatures; the use of colored prisms to distinguish inks of different colors, and others.

5. *Smithsonian Geographical Tables* prepared by R. S. WOODWARD. Washington, 1894 (Smithsonian Miscellaneous Contributions, No. 854).—This volume is the second of the series planned

by Prof. S. P. Langley to take the place of the earlier Meteorological Tables of Dr. Arnold Guyot, the fourth and last edition of which was issued in 1884. The appearance in 1893 of the first volume of this new series, which is devoted to Meteorological Tables, was then announced in this Journal (vol. xlvi, 160); the third volume, still to come, is to include Physical Tables. The volume now issued contains 105 pages of introductory matter, giving useful formulas, discussion of mensuration, units, geodesy, astronomy, etc. Then follow forty-two tables, chiefly geographical in object, and finally the work closes with the Appendix giving the relations of units, prepared by the late Mr. G. E. Curtis for the earlier meteorological volume.

6. *French Academy of Sciences.*—The French Academy has recently conferred the Janssen prize upon Professor George E. Hale of the University of Chicago in recognition of his important discoveries in astrophysics.

Bulletin of the American Museum of Natural History, vol. vi, 384 pp. 8vo, with 10 plates, 1894.—This new volume of the American Museum Bulletin contains a paper by H. F. OSBORN and J. L. WORTMAN, On the Fossil Mammals of the Lower Miocene White River beds; two by J. L. WORTMAN, On the Affinities of *Leptaretus primus* of Leidy, and On *Patriofelis*, a Middle Eocene Creodont; several papers by J. A. ALLEN. On Mammals from New Brunswick, On Mammals of Arunsas Co., Texas, On Cranial variations in *Neotoma micropus*, On Chilonycteris rubiginosus of W. Mexico, and On fifteen new North American Mammals; two papers by F. M. CHAPMAN, On Birds of Trinidad, and On Mammals from Florida; three papers by W. BEUTENMÜLLER, On some N. A. Ægeriidæ, On some N. A. Orthopters, On N. A. Moths, and a Catalogue of Orthopters found within 50 m. of New York; and a paper by R. P. WHITFIELD on new forms of Algæ from the Trenton limestone.

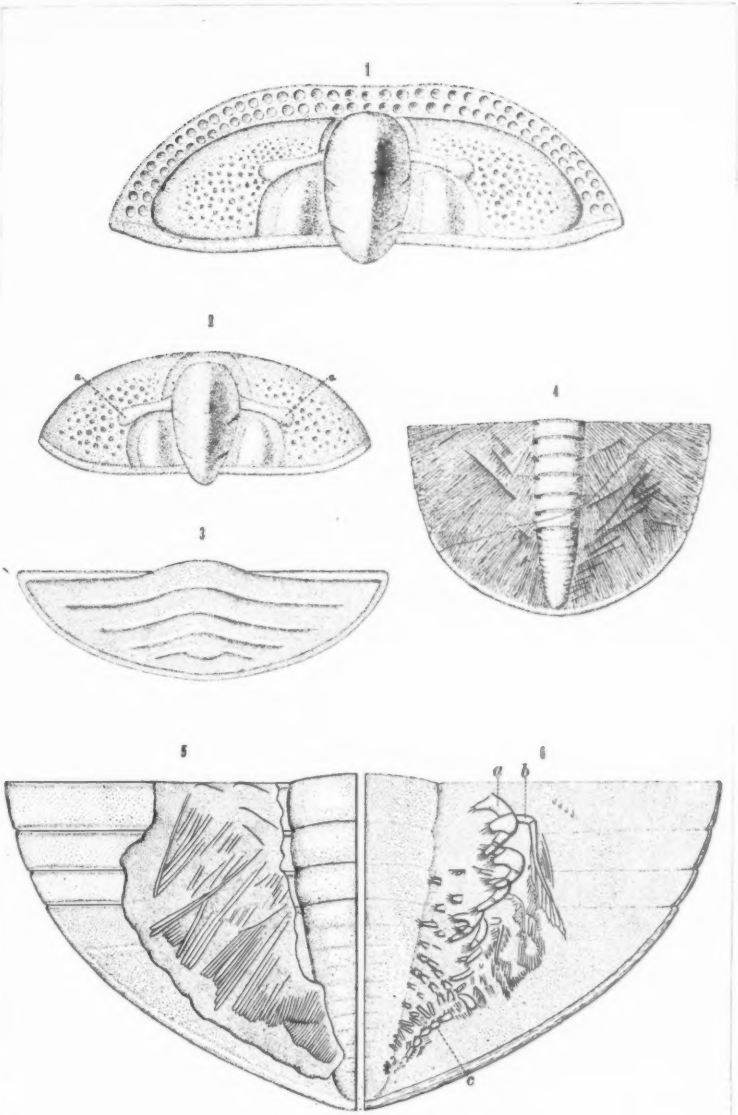
OBITUARY.

DR. GEORGE A. REX.—Dr. Rex, of Philadelphia died suddenly on the fourth of February last. The following paragraphs are from the Proceedings of the Academy of Natural Sciences of Philadelphia, of which he was a member.

Dr. Rex was the highest authority on the Myxomycetes in the United States. It was his enthusiastic study of this group that first brought him to the Section, and his communications on this subject formed an interesting part of nearly every meeting. He was the author of numerous species, which, owing to his extreme conservatism, will doubtless continue to bear his name. Many forms, new to him, remained in his collection unnamed for years, and were only published when he had thoroughly convinced himself that they were really new to science.

Although he was interested principally in the Myxomycetes, he was an earnest student of the lower orders of Fungi and an ardent admirer of everything beautiful in microscopic nature.

Recent deaths abroad are the following: MARQUIS DE SAPORTA, the eminent botanist, at Aix; Professor HEINRICH WILD, of St. Petersburg, well known for his researches in magnetism and optics; Dr. ALFRED W. STELZNER, Professor of Geology at Freiberg, on February 25th.



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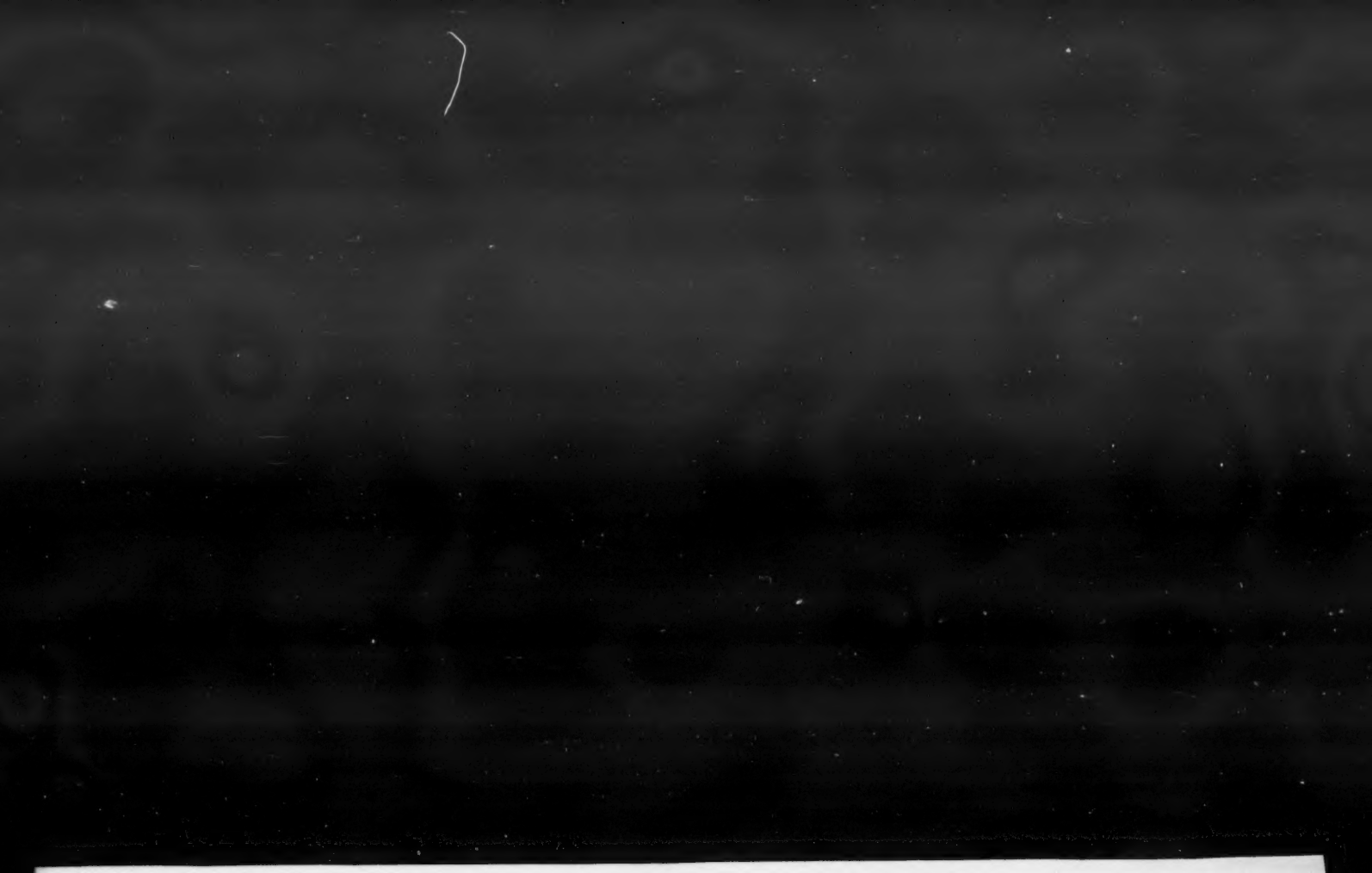
1, 2. Cephala of young individuals. ×40.
3. Pygidium of young individual. ×40.
4, 5, 6. Appendages. 4, ×4; 5, 6, ×11.